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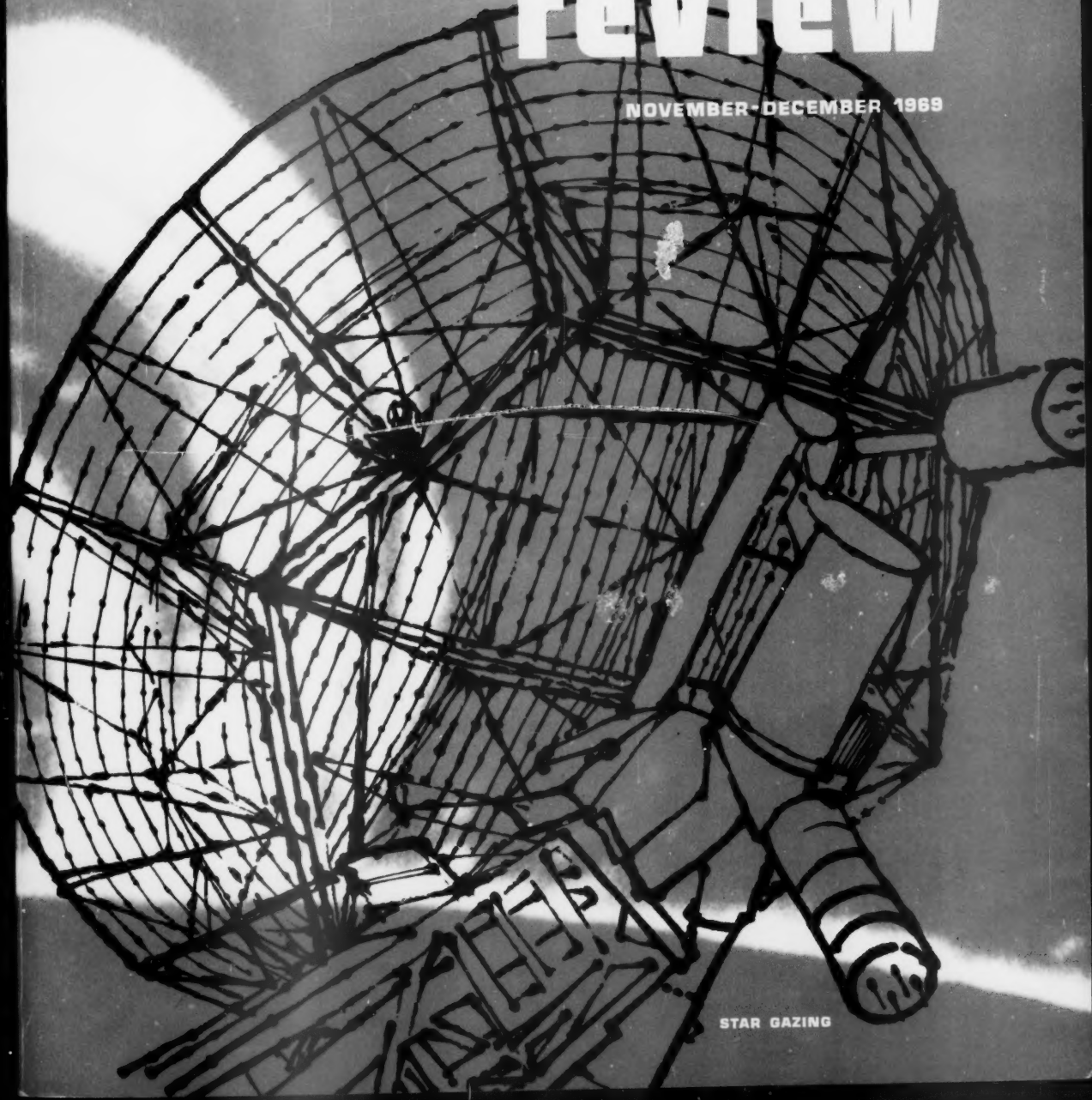

STAR GAZING

AIR UNIVERSITY
review

THE PROFESSIONAL JOURNAL OF THE UNITED STATES AIR FORCE

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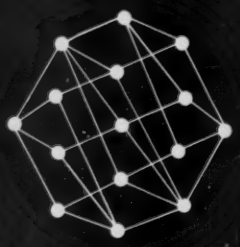
the cover

While mankind focused rapt attention on the moon in recent months, many astronomers, meteorologists, and physicists have been more closely oriented towards the sun and its activity. Colonel Dale J. Flinders explains the dynamic role of the Air Force and other organizations in this sophisticated "star gazing." Brigadier General Harvey W. Eddy, Commander, and officials at Office of Aerospace Research laboratories and offices discuss other far-ranging activities of the Command.



RESEARCH PLANNING AND COUPLING IN OAR

Brigadier General Harvey W. Eddy

THE AIR FORCE depends on a science-based technology to accomplish its mission, and the Office of Aerospace Research (OAR), a science-oriented activity, helps make it possible for the Air Force to exploit technological opportunities and avoid technological surprise by any adversary. It is important that Air Force officers understand the complex interaction between research—the accumulation of new knowledge and understanding—and the solution of Air Force problems. Understanding research planning and coupling is especially important because of the current public and private discussions on supporting research, with the associated questioning of the benefits that accrue therefrom.

Research includes all efforts directed toward increased knowledge of natural phenomena and environment and toward the solution of problems in the physical, engineering, environmental, and life sciences. Thus, by definition, it includes all basic and applied research efforts directed toward the expansion of knowledge in various scientific areas. Even military research does not necessarily include efforts directed toward proving the feasibility of solutions to problems of immediate military importance or time-oriented investigations and developments.

One of the difficulties of managing a research program lies in the articulation

of our convictions in meaningful ways. The tendency exists to describe the scope of our research in terms of the resources we put into the operation rather than in terms of the yield from that research. Our productivity is described in terms far more meaningful to us than to those who support us.

There is much to learn of ways by which we can make a more convincing case for science, but we are beginning to perceive the scope of the task. One of the stumbling blocks to understanding research processing is a valid but completely inadequate notion of the innovative process in which research interacts with technology in a mission-oriented organization. This notion prescribes that research of potential relevance to the mission-oriented organization be nourished. The selection of such research would have a connotation of research planning. As such research yields new knowledge or understanding, it is translated into technology, which then forms the basis for new company products or, in the case of the Air Force, new military systems. This translation constitutes coupling. The only real problem is to optimize the process with respect to the near and long-term utility of research which the Air Force nourishes—and to the extent possible, we undertake to do so in OAR.

No one challenges this model of the process, but it has been our experience that almost everyone challenges its optimization. Throughout the history of OAR the orientation of that challenge has posed particular problems. With possibly a few deeper historical sidelights added, it is sufficiently instructive to look at the general and particular assessments of the challenge to optimization as they have

existed since World War II. The general case is the national view of science, and the particular case is the national view of science within the Air Force.

Concerning the general case, science has only recently become the subject of political scrutiny. In the fifties, the national view of science appeared to be influenced largely by after-the-fact accounts of scientific exploits during World War II. It was a view that good men of science should be supported and left largely to their own desires because the utility of science is inevitable. For many years Congress seemed to go along with the premise that the future of the nation was dependent on large investments in science and technology primarily controlled by scientists and engineers.

This view gradually yielded to the more critical attitude that it was not enough to support good scientists and leave them alone. It was necessary to find ways to put research and technology to use. Research had to be exploited. This changing view clearly implied more economic constraints, which have been imposed increasingly in an unsettled environment influenced by a number of interrelated factors, including the imbalance of international payments, the skewed distribution of federal R&D funds among universities, the escalation of spending on Southeast Asia, civil disturbances, urban needs, the manifest restlessness of youth, and a seemingly growing antagonism to things military. There also seems to be a current aversion to investing in the solution of tomorrow's problems when apparently we have not invested sufficiently in today's.

As for the particular case of Air Force research, what more urgent military situation confronts the nation than Vietnam? Why not concentrate all available research into the support of technology pertinent to Vietnam?

The widely held notion of the innovative process is a valid model because it is an after-the-fact view of innovation. Donald A. Schon in *Technology and Change* says that we like this view because it tends to relieve uncertainty and convey the impression that innovation can be controlled, managed, and justified when in fact it is nonrational and uncertain.

This uncertainty is apparent from the results of a recently supported National Science Foundation study, "Technology in Retrospect and Critical Events in Science (TRACES)," which demonstrated that in a number of developments studied most of the key scientific discoveries necessary to those inventions were already made by scientists seeking knowledge in their respective fields without regard to applications long before management conceived of the technological development or the product.

If uncertainty is the general condition, then the greatest value of OAR is to stimulate a meaningful dialogue between the scientific and technological communities. Actually this is what coupling is all about. Even so, scientists at a recent symposium on coupling research and production concluded that the greatest need for innovation was not coupling but rather the motivation for coupling.

In any event, if we conclude that our model of the innovative process is only discernible after the fact, what is the value of planning? Is it worthwhile to formulate objectives which invariably will have to be modified in light of discoveries made in the process? What is the balance between the formulation of objectives and the need for flexibility? Is radical revision of a plan a confession of failure? Must planning be so general as to be meaningless? Is it not possible that the attraction to the rational view is so strong that plans which begin as flexible programming devices become myths about the process?

THESE questions concerning what we might call the certainty of uncertainty serve as a backdrop for what we consider coupling and planning in OAR. Our approach to coupling in this sense is to be found in the scope of our organization and activities. We use five principal means to help bring about a meaningful dialogue between science and the Air Force.

• The first means is through in-house research. The Air Force research program

encompasses research in the physical, engineering, environmental, and life sciences. OAR in-house programs are concentrated in the physical, engineering, and environmental sciences. Our Aerospace Research Laboratories (ARL) have significant research under way in both the physical and engineering sciences. OAR's largest in-house research laboratory complex, the Air Force Cambridge Research Laboratories (AFCRL), emphasizes research in the environmental sciences. At AFCRL research in the environmental sciences, however, is supplemented by their programs in both the physical and engineering sciences. Air Force scientists at our newest mission element, the Frank J. Seiler Research Laboratory (FJSRL) at the Air Force Academy, do research in both the physical and engineering sciences. Finally, the Office of Research Analyses (ORA) at Holloman AFB, New Mexico, does research in the physical and social sciences.

• The second means of bringing the products of science to bear on Air Force problems is through the support of external research with universities, research institutes, and industry. Most of this sponsored research is within the United States. Because there is a mutuality of interest, however, limited research is supported throughout the free world. Such research is supported in Great Britain, Western Europe, the Near and Middle East, and Latin America through the European Office of Aerospace Research (EOAR) and the Latin American Office of Aerospace Research (LAOAR). OAR also supports research in Canada and the Far East.

All Air Force-sponsored research is in one of two categories. The first category, comprising about 40 percent of the external effort, is research to complement in-house programs. This complementary research is directly related to current internal programs at ARL and AFCRL, plus a small amount in support of Air Force Systems Command (AFSC) in-house laboratories. This category of external and extramural research either extends internal investigations or provides for parallel approaches to internal investigations.

The other category of sponsored research makes up the Air Force Office of Scientific Research (AFOSR) program, which accounts for the remaining 60 percent of funds available to the OAR for extramural research. This category is predominantly under a program of research agreements, mainly university grants. The AFOSR program includes research in each of the broad areas of science enumerated earlier but performed in the national and international scientific communities. Thus AFOSR sponsors and administers an extramural program of scientific research relevant to Air Force interests. It is charged with helping to build a stockpile of knowledge that will provide the know-how for solving Air Force problems of the future.

Through these two means, then, the Office of Aerospace Research seeks to conduct and sponsor research in general areas of Air Force interest, with the expectation, of course, that the results may provide answers to Air Force problems—or forestall problems.

• It is clearly recognized, however, that the Air Force can never expect to do or buy all the research that is likely to be relevant to Air Force needs. Accordingly, a very important means of helping to exploit science for future Air Force technology is to be aware of all current research whose results may be pertinent to the Air Force. The extent to which the Air Force is involved in the conduct and support of research enables it to observe research by others. This is a third means of achieving a meaningful dialogue between science and the Air Force. It comes about through the continuing exchange of information and ideas in day-to-day contacts, in technical meetings, and through scientific symposia.

The doing, buying, and observing of research all relate to Air Force involvement with science. Who has the responsibility to bring science to bear on Air Force technology? Any notion that a research agency's work ends with publication of research results leaves the problem of finding and interpreting those results to someone else. The vast expansion of science has made communication even among scientific disciplines difficult. Communicating between science and technology is far more difficult. A recent study at the University of

Denver supported by the Advanced Research Projects Agency (ARPA) described the difficulty as due to natural barriers—costs, management, procurement, development, and even survival barriers. OAR tries to help reduce these barriers by becoming involved both with Air Force technology and Air Force long-range planning.

• Involvement with technology is a fourth source of rapport between science and the Air Force. OAR involves itself with Air Force technology in a number of ways. AFCRL, in addition to its environmental research programs, manages the Air Force exploratory development program in the area of environment. It conducts and sponsors other exploratory development in avionics and ground electronics in direct support of AFSC.

Whereas it has proven effective to have OAR research people participate in AFSC development programs, it has also been profitable for the Air Force to enable the development laboratories of AFSC to participate in other Air Force research programs. Accordingly OAR supports research in engineering and life sciences through a number of AFSC laboratories.

While our involvement with technology within AFSC is a principal means of helping to couple research to Air Force needs, it is not the only means. The ultimate effectiveness of OAR is the extent to which the Air Force research programs underlie a superior Air Force, and so we talk continuously with the operating commands of the Air Force to seek out problem areas or new concepts which can be assisted by the research talents available in OAR.

Visits to and from other elements of the Air Force are important sources of new ideas. Perhaps even more important is the exchange of people. In carefully selecting military people for assignment to OAR, we weight operational experience along with scientific and engineering qualifications. We also recognize the value of releasing research-experienced officers for Air Force-wide assignment. For comparable reasons we are interested in increased opportunities for the mobility of our civilian staffs.

Still another method of research involvement with Air Force and Defense technology is through consultation. Consultation is inherent throughout OAR activities, often in ways that are not apparent. The scope of AFOSR programs is an indicator: it currently supports about 1100 active research investigations involving some of the nation's best scientists. These scientists are available and have been consulted repeatedly, on short notice, to provide advice on unforeseen Air Force problems. More than 80 percent of the university scientists supported under the engineering sciences program serve as consultants to companies working for the Air Force or on Air Force-related problems. OAR-supported scientists serve on advisory groups to AFSC elements such as the Space and Missile Systems Organization (SAMSO), the Electronic Systems Division (ESD), and others.

Earlier I mentioned motivation for coupling. OAR experience with industry suggests that motivation is a strong factor in coupling science and technology. Much research is, of course, done in industrial laboratories and some of it is supported by OAR, but recent case studies within OAR reveal many instances where industry actively reached out so as to exploit Air Force research results. An example is the OAR-sponsored university scientist who developed computer programs to analyze and synthesize active circuit networks. Industry took this idea directly from a scientific journal report. Interestingly enough, these research results also found application through summer short courses conducted by the scientist and were in time incorporated into the curricula of a number of universities—all a good example of coupling between science and industry.

OAR in-house scientists consult with scientists and engineers in the more product-oriented elements of the Air Force, and laboratories have been located to facilitate communication. All OAR laboratories are tenants of other elements of the Air Force. ARL is located in the midst of the Aeronautical Systems Division (ASD) and other AFSC laboratories at Wright-Patterson Air Force Base. AFCRL is a tenant of the Electronic Systems Division of

AFSC. AFCRL also maintains an environmental consultation service. FJSRL is a tenant of the Air Force Academy (USAFA). ORA is a tenant of the Air Force Missile Development Center (AFMDC) of AFSC.

• The fifth means by which OAR undertakes to bring about an impact of science on Air Force technology is through education. This takes place through Air Force educational facilities and in a broader sense through the support of research in civilian universities.

OAR collaborates extensively with the two major Air Force in-house educational establishments, Air Force Institute of Technology (AFIT) and United States Air Force Academy. AFIT has a cooperative program in which graduate students at the master's level spend six months in residence at Aerospace Research Laboratories. During this period students do the research for their theses under ARL sponsorship. In addition, there are joint research tasks in which both ARL scientists and AFIT faculty members participate. In this way additional scientific talent is brought to bear on research of Air Force interest from both the faculty and students. Equally important is the working knowledge that the students receive of the Air Force research program. Not infrequently some of these students are assigned to one of the OAR laboratories upon graduation. Those who are assigned elsewhere in the Air Force carry that research familiarity with them, and it has the inherent potential for coupling science and technology. In addition to the master's program, there is a newer ARL-AFIT program for doctoral students in which officers spend four years in residence at ARL.

Seiler Laboratory was established as a cooperative undertaking of OAR and USAFA.[1] It is a small laboratory with predominantly military scientists. As in the AFIT-ARL program, faculty members and OAR scientists undertake joint research programs. FJSRL scientists do some teaching at the Academy. FJSRL provides a means whereby scientifically interested cadets may become better acquainted with Air Force research. This experience has significant value in future assignments of Air Force Academy graduates.

OAR also has an important role in the training of young officers who are assigned directly to the in-house laboratories. Usually these are officers with advanced degrees on their first Air Force assignment. They are a valuable source of scientific manpower in the OAR laboratories.

While the overall topic of DOD-university relationships is too broad to be treated adequately here, it is appropriate to touch on aspects of it as it relates to coupling. It is meaningful in this sense to consider that when we provide support to a university scientist we create a resource which has many facets.

First and foremost, OAR facilitates the exploration of an idea. When it decides to support a particular scientist, OAR does so on the basis of his scientific competence and the relevance of the idea to future Air Force needs. OAR then makes it possible for him to explore that idea. In that way the mission orientation is not employed to interfere with or to direct the activities of the research scientist. An important spin-off of the process is the support of scientific education on which the Air Force and Defense Department are dependent.

THE necessarily intricate concept of coupling, in contrast with the after-the-fact view of coupling implied by the idealized model of the innovative process, is pertinent to the OAR approach to research planning.

Because OAR is engaged in "phenomena-oriented research in a mission-oriented organization," it shares some of the semantic difficulties associated with planning that plague both industry and the government. OAR investigators try to dig deeper into the nature of some kinds of natural phenomena that the Air Force needs to know more about. We do this in hopes of solving some of the Air Force problems that we have today and are likely to have in the future. This kind of research requires special considerations in trying to plan it.

Phenomena-oriented research is somewhat a venture into the unknown, and hence planning is almost a contradiction in terms. This is not quite so, as there are ways in which

we can indeed plan certain related matters.

The OAR staff does planning with respect to the goals, products, and strategies of OAR. In a very broad sense, our goal is to be responsive to near and long-term Air Force needs.

A major source of long-range Air Force guidance is the publication, *USAF Planning Concepts.* This document, authenticated by the Air Force Chief of Staff, is the official source of broad guidance for all Air Force planning objectives and concepts resulting from actual or anticipated changes in world environment. This is a fifteen-year assessment of future Air Force needs in functional terms. Part IV, "Desired Capabilities," is of particular interest to OAR. OAR is a major contributor to the "Technological Horizons" section in Part IV, which identifies projected opportunities in six broad technological areas: Aerospace Vehicles, Electronics/Sensors, Computers/Information Processing, Environmental Sciences/Global Operations, Weaponry, and Life Sciences/Human Resources.

The OAR planning task is to extract the scientific needs from these desired capabilities. There is no magic formula for doing this planning, and we suspect that none will ever be found. However, searching for answers to these all-important questions in responsible and intelligent ways increases the contribution of our activities.

The OAR counterpart to *USAF Planning Concepts* is the Office of Aerospace Research Five Year Plan. It contains organizational research objectives directed toward the attainment of desired Air Force operational capabilities as outlined in *USAF Planning Concepts.* It includes the projected corporate development of OAR, a compendium of planned scientific and technical efforts, and an estimate of resources needed to accomplish the work.

While these published planning documents are only partial indicators of the extent to which we do planning, they are quite important. The preparation and use of the compendium of scientific and technical efforts are especially significant because they are instrumental in bringing many of the coupling mechanisms into play and illustrating ways in which research coupling and planning blend together. The extent to which research planning forms the basis for day-to-day decision-making requires that such planning be iterative. Accordingly, objectives have to be revised as the current scientific base evolves and as Air Force objectives are altered by the dynamics of the national and world environments.

Overall, Air Force scientific and technical objectives are examined continuously and altered as appropriate at least annually. Key OAR scientists participate on each research panel along with members of the headquarters scientific staff. The AFSC Director of Laboratories also designates a representative for each of these subelement panels. This is a particularly effective method for insuring that Air Force technological needs are considered in the research planning process.

The OAR Five Year Plan is given wide distribution within DOD, and the scientific and technical objectives portion of it, entitled *Air Force Research Objectives,* is given much wider distribution—to universities, research institutions, and private industry. This dissemination of Air Force research interests has the twofold purpose of stimulating participation in the Air Force research program and of encouraging the exchange of information so vital to the scientific community.

In support of these broader objectives, the best scientific talent within the Air Force is brought to bear directly on the OAR planning process. Additionally, the use of subelement panels provides a forum for constructive involvement of laboratory scientists in the management of OAR; a feedback loop from Air Force needs to science; and a natural tie-in with Director of Defense Research and Engineering (DDR&E) discussion forums, which include scientists from the Army, Navy, and Air Force.

The choice of research program areas and of research projects within those areas is considered a part of planning. In this context, incidentally, an admonition of Russell L. Ackoff and Maurice W. Sasieni, in *Fundamentals of Operations Research,* is appropriate: There are no such things as physical problems, en-

gineering problems, environmental problems, and so on; there are only problems, and the disciplines of science represent different ways of looking at them.

THE SCOPE of OAR involvement in the four major categories of Defense research sciences and in thirteen of the fourteen scientific subelements[2] gives a number of apertures for looking at Air Force problems.

The Air Force research program is structured through the choice of fields in which research is to be supported. The availability of competent scientists to do research within those fields is a major factor in the selection of individual research efforts.

While there is no substitute for scientific competence, there are other prerequisites to a successful research program; and experience has shown that certain of these can only be achieved through planning.

It seems that there has to be a "critical mass" of capability in each of the fields of science mentioned previously in order to make the involvement meaningful to the Air Force. There also should be an adequate range of disciplines represented in the organization, to cover the general areas of scientific interest to the Air Force.

Because resources—money, facilities, and, most important, people—are necessarily limited, planning must maintain a balance between scope and depth of coverage. This is largely a matter of judgment by experienced and well-qualified research administrators, as is the balance between involvement with technology and freedom from excessive involvement in detailed, short-range engineering problems.

OAR scientists have contributed significantly to the support of military operations in Southeast Asia—for example, by developing sensors, a whole family of compact weather instruments, and personnel rescue techniques. Most of the OAR research resources are assigned, however, against longer-term Air Force needs.

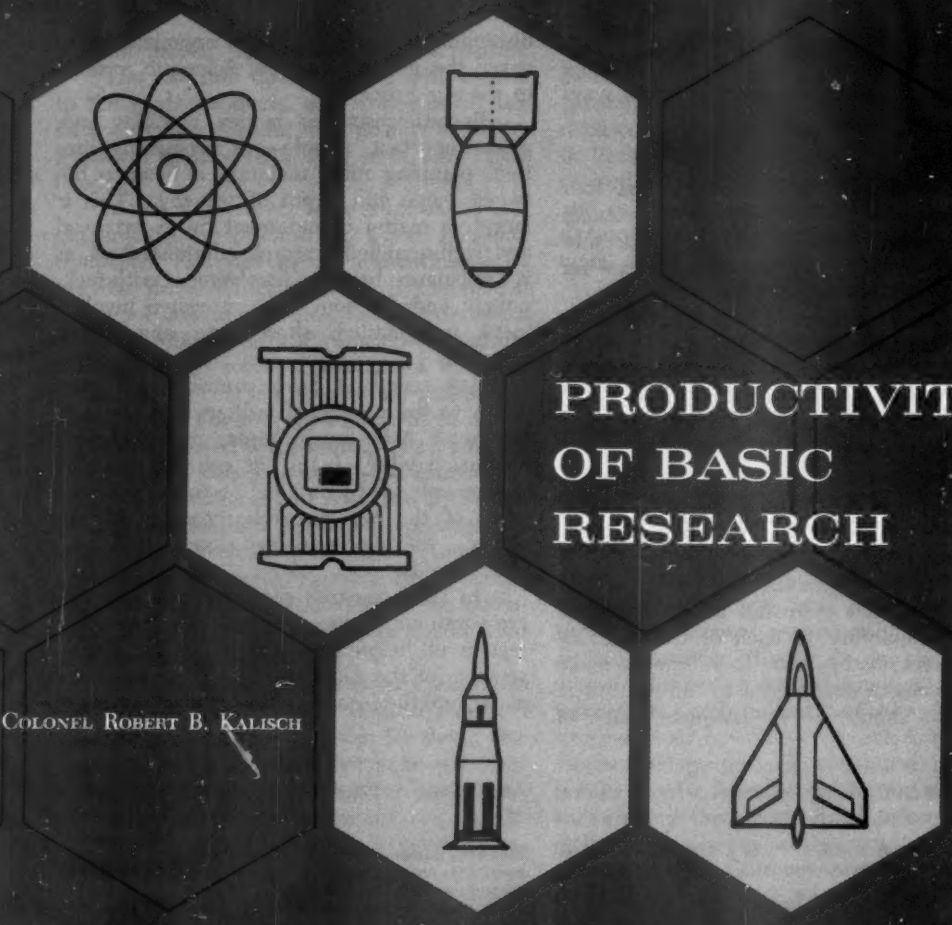
In OAR, research planning and coupling are continuous processes, with the overall objective of helping to secure the benefits of science for the Air Force, for the present and as far into the future as it is possible to project.

Office of Aerospace Research

Notes

1. See Gage H. Crocker, "Frank J. Seiler Research Laboratory," *Air University Review*, XX, 1 (November-December 1968), 40–48.

2. OAR has projects in general physics, nuclear physics, chemistry, mathematical sciences, electronics, materials research, mechanics, energy conversion, terrestrial sciences, atmospheric sciences, astronomy and astrophysics, biological and medical sciences, behavioral and social sciences. It has none in oceanography.



PRODUCTIVITY OF BASIC RESEARCH

COLONEL ROBERT B. KALISCH

THE national ability to transform scientific opportunities and technological capabilities into military force is a critical part of national strength. The emergence of nuclear power, nuclear weapons, modern electronics, missile technology, and jet aircraft are examples of areas in which technology has altered warfare in the past twenty-five years. Clearly, the need exists to incorporate technological innovation into the military structure. The issue is to insure that this process has the vigor and creativity necessary to maintain the national military superiority.

The task of creating and maintaining technological military superiority is partly in the realm of technology and partly in the realm of science. The realm of technology is the province of the Air Force Systems Command. The realm of science is the concern of the Office of Aerospace Research (OAR), and this article will review the nature of the OAR contribution to the Air Force.

Some aspects of the Air Force involvement in science and basic research are quite controversial. It is almost universally accepted that the industrial-military complex must work from a base of outstanding scientific research. Similarly, it is widely accepted that this scientific base requires independence of thought and motivation for growth and strength. The focal issue of the debate is, At what point in the research process and how should the Depart-

ment of Defense involve itself in the work of science in basic research?

Several fundamentally different ideas emerge from the literature.[1] On the one hand some believe that involvement must be in every stage of research. They favor direct support of basic research by the individual services. Many arguments are brought forth in support of this point of view.[2] These arguments generally bring out the difficulty of transferring knowledge from science to technology, and hence programs that involve both the scientific community and the military will produce more significant results.

The standard counterargument emphasizes the independence of the basic researcher. His freedom is the essence of his creativity. He should be provided an environment that permits him to work productively. Then the military or any other consumers of science can skim off the useful science required by their needs. This approach envisions a National Science Foundation or its equivalent motivated by building a national capability for fundamental inquiry supporting universities and research centers so that science flourishes in the country.[3] Some equitable means for determining support levels in the various disciplines would be developed to reflect national needs.

At issue for the mission-oriented activity is the corporate research function. It is this job of interfacing science to Air Force-oriented technology which dominates our problem. An organization such as Office of Aerospace Research transforms discipline-aligned science into task-oriented technology and vice versa.

the program

Basic research is "research directed toward the increase of knowledge, the primary aim being a greater knowledge or understanding of the subject under study."[4] It is differentiated from exploratory development in that a usable product or end item is not yet identified in basic research. In exploratory development at least the general class of a military system is well defined and related to an individual work unit.

An equivalent point of view is that exploratory development is product oriented and basic research is phenomena oriented. In this jargon, basic research emphasizes an understanding of the world around us in contrast to the interest of exploratory development in creating useful devices or techniques.

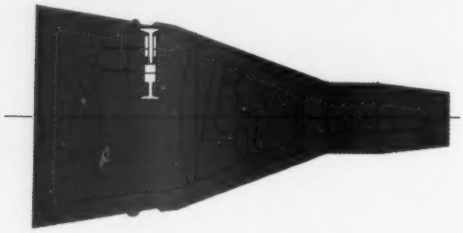
The Air Force has since its inception supported basic research.[5] Although the dollar investment has varied, it now is about $100 million annually. For fiscal year 1968 the Air Force planned less than three percent of the research, development, test, and evaluation (RDT&E) budget for basic research. A prescription for deciding the optimum level of support to basic research has not been found. Some organizations simply allocate a set percentage of the operating revenue each year for research. In contrast the Air Force funding level varies from year to year with no set formula.

Since the financial investment in basic research is relatively small, why is the interest in it so high? Simply stated, the radically new and unexpected changes in military capability will first be visible in this part of the program. As new physical knowledge and understanding are created, new opportunities for military exploitation will emerge. Such knowledge and understanding are the heart of basic research.

relevance

One of the semantic problems of the military basic research community is an understanding of the term "relevance." Without exception, managers and scientists demand that USAF programs be relevant, but agreement on what is relevant is much less complete. In examining the question of relevance, one can usefully define two extreme viewpoints. First, one might consider an esoteric project designed to gain a more complete understanding of the materials present on the solar surface. Such an investigation might be questioned as not being relevant to the needs of the Air Force. A critic of this sort may encourage study of electrical energy sources suitable for handheld radios on grounds that this is relevant, e.g., downed airmen need better batteries in their radios, etc. To this individual the more immediate the Air Force application of knowl-

The ion sensor, developed by AFCRL to sense the attitude of a spacecraft by detecting the device's orientation relative to the stream of ions through which the craft moves, was part of the instrumentation for Air Force Experiment D-10 on Gemini 10 and 12. On the latter flight it was put to good use when the inertial system was found to draw excessive power.



edge, the more relevant the research.

At the other extreme one might argue that basic research is phenomena oriented, not applications oriented. If the end product is envisioned, the project is properly a part of the exploratory-development program. Therefore, any study of batteries for radios is not basic research at all. This is the dilemma. If a study is relevant to the degree that it is development oriented, it is a misuse of basic research resources to perform the work as a part of the basic research program. If on the other hand the work is not related in a meaningful way to DOD needs, then the expenditure of DOD research money on it is unwarranted.

The basic research program manager is under pressure from both sides. These pressures force him to seek a logical relationship between the phenomena under study and the Air Force. Yet he is also sensitive to the programs that are more logically a part of exploratory development. Hence, a balance is created. To be sure, this balance moves in the direction of more immediate application during periods of military conflict. In periods of less tension the goals of such a program tend to become more distant, and the specific work units tend to become more fundamental in character.

productivity measures

In a democratic society each consumer of national resources must justify his level of consumption, and military basic research is no exception. The Congress, the DOD, and the public demand accountability. A measure of the productivity of basic research is required, to balance its needs against other demands for public funds.

A valid but somewhat trite justification is that, since the military uses science, they should support it. This argument has now been translated to the national scene. At that level the national need for science and the funding of the National Science Foundation are debated. The military support of science is a much more specific issue and must be argued on a narrower set of issues.

The need today is to relate a military capability to our basic research expenditures. In other words one needs a measure of the productivity of the basic research program. Such a measure evolves from a sophisticated view of the contribution of basic research. Let us discuss four such measures.

THE MISSION of Air Force basic research is to create opportunities for exploitation of science. This mission statement makes the evaluation of progress difficult. First of all, it is difficult to measure opportunities. Second, many of the research tasks are aimed at the creation of opportunities in the future. Finally, not all opportunities are of equal significance. How, then, does one measure the accomplish-

ments of a basic research program? The thesis advanced here is that this measure has at least four dimensions, four classes of achievement:

(1) the creation of new fields of endeavor,

(2) the applied science innovations which emerge from the program,

(3) the relevance to the USAF of the body of knowledge within a discipline,

(4) the ability of science to relate to Air Force problems and the Air Force understanding of science.

creation of new capabilities

By far the most dramatic military contribution made by basic research is the discovery which results in a new field of military capability. Nuclear energy is a noteworthy example. In the space of ten years a totally new concept of military power came into being as a result of this new science and its related technology. Not every new area created by basic research has had such far-reaching implications. Thus less-revolutionary examples may serve as more typical.

The maser and atomic clocks. In the postwar research program at Columbia Radiation Laboratory, Professor C. H. Townes studied microwave spectroscopy under the support of the Air Force Office of Scientific Research. His experiments involved the excitation of gas molecules and observation of radiation from excited gases. He noted that under certain conditions the molecules decayed from their excited state (they disposed of their excess energy) by emitting electromagnetic waves in the microwave region.[6] His most provocative discovery was that the microwave radiation could be made coherent. That is, intense "pumping" of a gas caused each excited molecule to radiate microwave energy in coordination with the others, thus adding to the magnitude of the radiated wave. This remarkable research led to the award of the Nobel Prize in Physics to Professor Townes in 1964. It also created the new area of practical quantum electronics that led to the development of lasers and atomic clocks. The significance of this research to military problems is profound.[7] New target-marking systems, operational time standards, geodetic measurements, and countless other military capabilities have come from this research.

The impact of quantum electronics on the art of time keeping is seen in the atomic clock, which uses a hydrogen maser to attain accuracies once believed impossible. If someone had asked the experts in time standards to improve the accuracy of clocks in 1946, not one of them would have considered microwave spectroscopy a relevant field. However, a better understanding of hydrogen molecules and their associated quantum energy states led to a capability far beyond that that the early postwar state of the art could have achieved. Again basic research had created a totally new area of technology.

Ionospheric scatter systems. Another basic research project that created a new area of military endeavor began at the Air Force Cambridge Research Laboratories (AFCRL) in the early 1950s. The operational problem was to extend the range of electronic communications systems and radars. The difficulty was that the frequencies in use were limited to line-of-sight range. Scientists at AFCRL suggested that the ionosphere might be used as a reflector or scatterer.[8] The scientific problem was to understand electromagnetic wave interactions with the ionosphere. As a result of their ionospheric and electromagnetic studies, they were able to propose radar and communications systems that operated over the horizon. This research was a foundation for the worldwide communications networks and very long-range radars that have so changed military operations. Many scatter communications systems are operational today. Over-the-horizon radars are expected to revolutionize the air defense problem for North America within the next ten years. Although the over-the-horizon electromagnetic systems have been a massive team effort of industry, university, and in-house laboratories, the AFCRL pioneering effort in this field helped the Air Force to an early understanding of the potential of such systems.

Optimal filters. Still another area of active Air Force development effort began with basic research on optimal filters. In 1958 the Air

Air Force research involving measurement of the human vocal tract and vibrations of the vocal cords to produce sounds led to a simple diagnostic method for early detection of laryngeal cancer. Air Force testing, and further research under a contract awarded by the U.S. Public Health Service, produced a feasible $1 technique: the subject's voice is recorded, and the waveform is analyzed by computer; certain vibration irregularities indicate a growth on or near the vocal cords.

Force Office of Scientific Research initiated support of the Research Institute for Advanced Study (RIAS) to investigate the use of modern mathematical statistical methods in estimation. This work was motivated by what appeared to be an opportunity in science for the creation of new techniques that could alter control applications. Under this support arrangement Dr. R. E. Kalman and Dr. R. S. Bucy wrote several papers[9] that revolutionized the area of estimation. The Kalman filter, as it is now called, has so affected the science of information handling, estimation, and decision-making that it might well be thought of as the creation of a new area of science. Specific applications of the Kalman filter include the decision-making element of Lockheed's new C-5A navigation system. This filter design theory relates several redundant data sources so that an optimum estimate of some parameter can be derived. A recent article in one of the trade journals describes the impact of the Kalman filter:

> Kalman-filtering applications have had an enormous impact on the design and development of the next generation of precise navigation systems. This technique of combining information from multiple sensor sources has achieved accuracies that clearly constitute what is a major breakthrough in guidance technology.
>
> The growing uses of optimum filtering or Kalman filtering have forced a large number of engineers to become at least familiar with the vocabulary used by the practitioners of the art. For many, the basic principles of Kalman filtering lie shrouded in mathematics and mystery somewhere out in state space. This condition is unfortunate, since engineers tend to distrust and avoid using methods which they do not understand. As a result, a significant part of the effort in the application of Kalman filtering has consisted of trying to convince prospective customers and program managers that these methods are not as complicated as they appear—that they really do work when properly designed and mechanized.[10]

applied science innovations

Probably the most visible class of accomplishment in a basic research program on a day-to-day basis is the applied science innova-

tions. These innovations occur because the phenomena-oriented scientists are keenly aware of military needs. Often a military need can be satisfied by the application of their newly discovered knowledge of some phenomenon. In DOD programs the scientists are encouraged to help satisfy such needs. These spin-off applications of research are another manifestation of the value of the program, although they do not represent a primary objective. Examples of this type of applied science dividend are featured each year in the OAR Research Applications Conference.[11]

An ion sensor. A significant application of the ion sensor was described at the 1967 OAR Research Applications Conference.[12] An important ion sensor was developed at the Air Force Cambridge Research Laboratories. This research was motivated by a desire to measure ion concentrations in the upper atmosphere. As the research progressed, it became clear that, in addition to detecting ions, an ambient positive ion sensor could be used to sense the attitude of a spacecraft. This additional capability was developed into suitable satellite instrumentation. The device offered such promise that it was made a part of the instrumentation for Air Force Experiment D-10 on Gemini 10 and 12. A remarkably short time between the creation of this instrument and its practical utilization was realized on the flight of Gemini 12. On this flight, fuel-cell degradation late in the flight prompted Mission Control to abandon use of the inertial system and depend on the new ion sensor system.

Besides the advantage of having a lower power requirement than the Gemini inertial system, the positive ion sensor system has merit in its own right: its system response time, weight, lifetime, volume, and cost are all improved over the inertial system. For example, the ion system costs $30,000, against $1,500,000 for the inertial system. These advantages illustrate the impact that this new attitude-sensing system will have on future spacecraft and possibly on supersonic aircraft.[13]

This example shows how the motivation

In 1969 AFCRL's basic research produced the backfiring antenna, which became the primary antenna used in the Tactical Satellite Communications System. A novel antenna, it is simple to build and easily transported to any area.

of basic research can provide a totally new approach to an operational problem. If basic research had not required the measurement of ambient positive ions, no such instrument would have been created. The coexistence of the scientific problem, the knowledge of spacecraft needs, and the mission to solve these problems motivated the invention. The Air Force Cambridge Research Laboratories are often in this position. It is this "middle ground," with interests in science and in Air Force problems, which enables contributions of this sort. It is this type of work which emphasizes the Air Force need to be involved in basic research.

Plasma display panel. Another class of innovation that has been a product of a basic research program is the plasma display panel. This new technique for information display was developed at the University of Illinois under partial support by the Air Force Office of Scientific Research through the Joint Services Electronics Program. The investigation was motivated by a broad laboratory research task, designated Programmed Learning and Automated Teaching Operations (PLATO). To facilitate machine teaching, several student programmed-learning setups were to be used, each one requiring a display device cheaper and more flexible than a typical cathode-ray tube.[14] The revolutionary idea was to use a series of small cells arranged in a square matrix, with illumination originating from a plasma discharge. Basically, the panel is an array of cells filled with a gas which, when ionized, will emit light of adequate intensity. Leads for digital switching of individual cells are attached to the sides. Each panel is a three-layer sandwich of thin glass sheets, the interior sheet having a rectangular array of holes in it. These holes form the space that is evacuated and then filled with gas.

The advantages of a plasma display panel over the cathode-ray tube (the standard display device) are lower cost, greater reliability, no inherent size limitations, and color flexibility. In addition to the typical display requirements already cited, the plasma display panel provides some capabilities that are not possible in other devices. One potential is for a genuine three-dimensional display; as sheets of plasma display panels are fitted together, a three-dimensional display volume can be created. Another potential is for multicolor display.[15] One of the greatest inherent advantages of the plasma display unit is its ability to handle digital signals. This means that a computer output can be directly fed into the display system without intermediate processing into analog form.

Both the ion sensor and the plasma display unit result because basic research had motivated scientists to do something radically new. In an effort to count ions more exactly and to provide a more suitable display for computer-aided instruction, scientists solved the problems in a new and imaginative way. The result in each case is an innovation which provides capabilities far beyond the current state of the art.

A most important point relative to this type of contribution of basic research is that these applications were not the primary motivation of the research. In each instance another problem was under study and the accomplishment was a by-product of the research. It is essential that these by-products, however important, not be confused with the true motivation for the basic research.

technical education

Since basic research is the pursuit of knowledge, its intimate relation to education is not surprising. The fact that a large portion of basic research is performed in the academic environment emphasizes the closeness of education and research.[16] At the graduate level, of course, there is no difference. Thus the Air Force is greatly concerned with education. One manifestation of its concern is the Air Force Institute of Technology program designed to provide an understanding of modern science. This reflects the Air Force commitment to science. The modern officer must be able to grasp the military implications of scientific and technological advances.

Important as a sound scientific foundation is to the military officer, it may be even more important to his military-industrial counter-

part. The Air Force basic research program is involved in a real sense with the education of future aerospace scientists and engineers. Basic research causes the relevant disciplines to grow rapidly. An example of this phenomenon is the field of electronics, which has been supported by the military for over twenty-five years. The success of such support is indicated by comparison of the pre-World War II electronics engineer's capability to solve military problems with that of today's electronics engineer. Television, solid state devices, infrared techniques, and modern control theory are just a few examples of entire technologies that have been created.[17] Each area owes a major part of its growth to basic research supported by the Department of Defense. These efforts over the years have created a body of knowledge that provides the university graduate with an improved capability to solve military problems. In this way basic research contributes to military capabilities by improving the base of knowledge in selected relevant disciplines.

The growth of knowledge pertinent to the design of antennas is a clear example of this influence. Military needs have dominated antenna design since World War II. As new theories and techniques of design began to emerge in industry as well as in the academic community, vigorous support by the military insured that this body of knowledge would be expanded rapidly. This was true at Harvard and Ohio State, to mention only two universities involved. The vast body of relevant knowledge exists today because the military supported relevant research. The payoff is the technical knowledge now imparted to engineering students which enables them to design military equipment more effectively.

window on science

Broadly based basic research oriented to a military program provides a window on science. By maintaining a knowledge of new opportunities and capabilities, the Air Force avoids being surprised by scientific developments. In a period of history when science can interact with technology so quickly and create totally new military capabilities, the Air Force cannot allow itself to become out of date on scientific progress. This intimate dialogue with the pertinent disciplines insures that Air Force problems are under scrutiny by the leaders on the frontiers of knowledge.[18] The role of scientific leaders as advisers to the Air Force has been important. Their participation in the planning activities permits the Air Force to make a selection from a broad set of alternatives to solve a given problem. Furthermore, this broad set of alternatives includes the most recent discoveries on the frontier of knowledge.

Traditionally within the military structure, prevention of surprise has been an intelligence function. In the Air Force, technological sur-

Three unusual antennas on the atomic carrier Enterprise *had their origin at AFCRL in the late forties and early fifties. Two of them dictated the distinctive flat sides and dome structure of the carrier's island. A smaller one (not visible) establishes glide paths for landing aircraft.*

prise was recognized as a type of risk requiring very specialized personnel and techniques.[19] This area of technical intelligence has been of steadily increasing importance for the past fifteen years. In a similar way the problem of scientific surprise has started to emerge. The technological problem was to understand and assess the threat from technological capabilities manifested for the most part in hardware. Aircraft, weapons, missiles, and energy sources are typical topics of technological concern. Now with the rapidly advancing frontier of scientific knowledge, a risk exists in that a scientific discovery could occur, accelerate, and change a technological area so quickly that the international balance of power could be upset. Fission and fusion weapons were two such instances. Coherent energy sources (lasers and masers) could conceivably be a third instance.

In these extremely high threat areas, the Air Force has no alternative but a serious commitment to understand new advances in science. This commitment must be made by an integrated team of civilian, civil service, and military scientists and engineers. It must include behavioral science, physics, chemistry, and any other discipline capable of a great impact on military power. It must also be worldwide in scope.[20] Such a program exists today. It is the Air Force basic research program committed to alerting the Air Force to new opportunities for extensions of military capability.

Office of Aerospace Research

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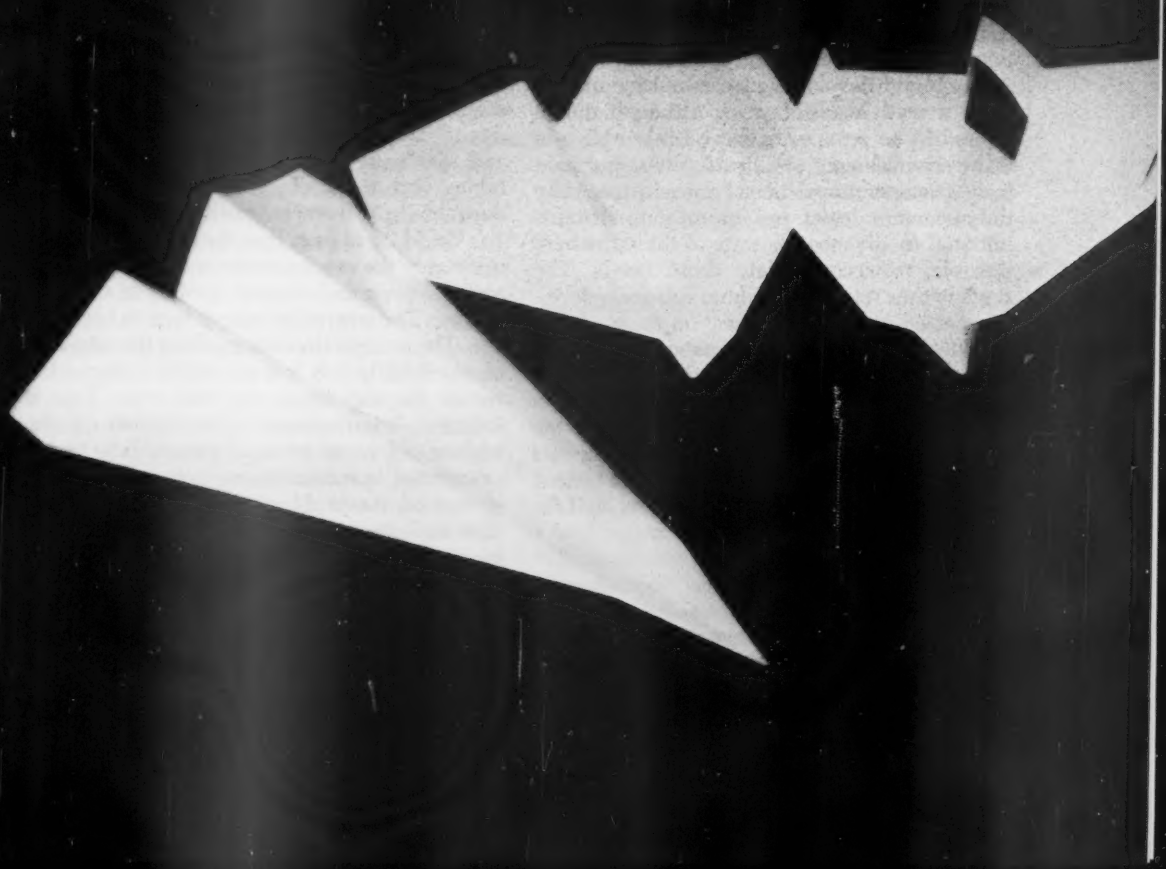
NECESSARY ELEMENTS OF AN EQUIPMENT DEVELOPMENT AND TEST PROGRAM

ANDREW S. CARTEN, JR.



NEW AIR FORCE equipment embodying advanced concepts almost invariably owes its existence to the fact that a scientist or engineer had faith in a novel idea. He nurtured the idea to the point where feasibility was demonstrated, and he communicated his success to the ultimate user, who saw in the idea the answer to an operational need. A series of administrative and technical actions then followed, which culminated in the delivery of the fully developed equipment to the field, complete with supporting data, spares, and ancillary items.

One might ask how the scientist or engineer happened to be working on an idea which so nicely satisfied an operational need. This is the ideal situation, of course, from both the developer's point of view and that of the user. In the real world, it seldom happens that operational personnel can pose a problem to the research and development community and receive a ready-made answer. Although this is an end to be pursued, a more achievable aim is the establishment of a development program in which operational needs are anticipated to the maximum extent and appropriate action is initiated to advance the state of the art, where possible, to accommodate those needs. The stage is thus set for a potential full-scale development effort when the occasion arises, and a solid technical base is established for the overall program. For a program of this type to have any hope of fulfillment, it must be geared to a series of realistic goals. This brings us to the four essential elements of a truly successful development program: the technical objective, feasibility, the requirements statement, and the development plan.

technical objective

The first essential element is a technical objective (TO), a published statement of specific goals in the exploratory development area. If a scientist is seeking to advance the state of the art in visibility measurements or in antenna design, for example, he is doing so in response to TO's stating that advances in those fields are required. The TO, in effect, becomes the charter under which he conducts his investigation.

TO's are general in nature and are not particularly adaptable to time phasing. This is in keeping with the independent, free-standing character of the research and development process and with the wide geographic and administrative separation between developer and user which the Air Force structure imposes. Nevertheless, TO's are expected to relate to operational needs. The exploratory development projects built around them are approved on that basis. The *degree* of relevance is the key issue. The TO's could mirror exactly and exclusively the formally expressed needs of the user, but this tends to be a self-defeating process, since scientists become preoccupied with day-to-day operational problems in this case and the "ivory tower" atmosphere disappears, taking with it the objectivity and detachment desirable in R&D work. On the other hand, the TO's could be so pure that they bear little or no relation to the user's present or future situation, and the program's output is of little value to anyone. The answer lies somewhere in between.

The catalyst that brings about the adoption of worthwhile TO's is meaningful dialogue between the developer and the user. This is informal intercommand coordination of the highest order, consisting of almost daily verbal or written communication between the two groups on the problems and findings of each. This dialogue is best carried out by spokesmen for the two groups, each knowledgeable in his own area and able to speak for his group.

The developer and user see life from two entirely different viewpoints. The developer takes a detached view of operational problems.

He looks at them academically as an endless challenge to his ingenuity and imagination but of no real concern to him on a day-to-day basis. He visualizes new approaches, new instruments, new observational platforms in terms of their functional excellence, without worrying about how they will be paid for and maintained. His mind races ahead five or ten years as a matter of course. His special awareness of advances taking place in related technical fields allows him to project into being techniques of which the user usually has limited knowledge.

The user, on the other hand, is generally overburdened with daily operational crises, sometimes global in nature. He is hard put to maintain his existing capabilities because of personnel turnover, and he is under constant pressure to acquire new capabilities. His operational equipment is usually a collection of old and new devices, many of them plagued with deficiencies which reduce their reliability and usefulness and further aggravate his problem of maintaining his capability at an acceptably high standard. He is painfully aware of the long time required to get newer, improved equipment into the inventory. Thus, his viewpoint tends to favor the present time period and the immediate future. Under these circumstances, his five- to ten-year planning documents can very easily be either mere projection of existing techniques or unrealistic expressions of advances that will never materialize.[1]

If a close exchange of information flourishes between the two groups, the user can maintain a realistic awareness of the latest state of the art and can plan intelligently for improved mission capabilities. He is encouraged to widen his horizons to include promising new techniques, of which he is not fully aware. From the developer's point of view, a continuous window is made available to the operational world which illuminates his investigations and keeps them in touch with—although not limited to—actual problem areas. The developer's technical objectives, by virtue of this dialogue, are scaled to the heights to which the user may wish or may be persuaded to climb. Priorities can be agreed upon which will discourage the developer from devoting his resources to efforts of no ultimate value to the user.

Relating these conditions to the exploratory development process shows that the essentiality of the technical objectives lies in the degree to which they provide practical guidance to the development effort and shape its output into a form compatible with the interests of the user. The ideal objectives are those which create and sustain the desired solid technical base from which will emanate a steady flow of advanced techniques and equipment capable of upgrading the mission effectiveness of the user. (The phraseology employed here permits considerable flexibility in the coupling of the R&D effort to the user's needs, but this is highly desirable. It allows the scientist to consider all possible solutions to an expressed need and guarantees the introduction of new ideas.)

The dialogue informs the user of the proper moment to begin the administrative actions required to bring specific new equipment into the inventory. This comes about in two ways: the developer may conclude that it is in the interest of the user to adopt a new technique whose feasibility has been established after extensive investigation but for which there is no officially stated requirement; or the user may seek the developer's opinion as to whether equipment embodying a new technique is feasible and capable of development.

In the following discussion of feasibility, which is the second essential element in the planning process, I shall draw upon my experience in the meteorological equipment development field.

feasibility

It is an oversimplification to say that a new technique is feasible because it can be shown to work. Mere performance of the specified function is an insufficient basis on which to determine feasibility. There are many additional considerations: Are the conclusions reached with respect to successful performance based on statistically meaningful test results? Where instrumentation is involved, are the measurements obtained by the new technique accurate and repeatable? Do they cover the entire range of interest? Is there any significant lag or hysteresis? How are the new measure-

ments related to measurements taken with standard instrumentation? Is there a diurnal effect? Are the outputs compatible with standard data-handling procedures? Can the technique be embodied in instrumentation that is reasonably priced for the intended application? Can the proposed instrumentation be easily calibrated? Can the calibration be maintained? And so on.

These questions indicate the depth and the importance of the feasibility concept. They are part of the check list that a scientist must apply to new techniques before he can declare them feasible or within the state of the art.

Determination of feasibility is mandatory before serious consideration can be given by the user to requesting any development involving a new technique. Thus, feasibility is more than an essential element in the development process. It is actually the keystone. If the user seeks to employ an unproven technique, the required development effort will be experimental and risky, requiring a state-of-the-art advance. On the other hand, if feasibility is clear, the resultant full-scale engineering development will be straightforward and safe, with predictable milestones.

It follows that the feasibility determination should decide whether exploratory development or engineering development funds will be allocated for a particular effort, the type of managerial controls to be employed, and the long-term or short-term nature of the development. If the feasibility concept is compromised and a technique is erroneously stated to be qualified for full-scale engineering development, despite the absence of demonstrated feasibility, an unfortunate malassignment is the inevitable result. Development milestones and cost figures are established which prove to be meaningless, and the user, in most such instances, receives either no equipment or equipment that is late, underbudgeted, and often less than satisfactory. It cannot be emphasized too strongly that feasibility *must* be established if the engineering development program is to be successful in terms of end-product quality, reasonable production lead times, and budgetary control.[2]

The feasibility check list should be applied before any experimentation is done. By this procedure many initially attractive ideas which arise in the course of the developer/user dialogue can be eliminated or declared feasible at virtually no cost. This is made possible by using the fund of knowledge accumulated by the developer. Some ideas, of course, are not classifiable by the check-list type of screening process. If they are agreed upon as desirable technical objectives, they should undergo experimental verification, and the possibility of development failures must be considered in planning the eventual use of the techniques being investigated.

An intermediate step is sometimes required to bridge the gap between exploratory and engineering development. This step is called Advanced Development and is particularly useful for those situations where a proposed system's feasibility cannot be truly proven in exploratory development because of financial and time constraints. It allows a concerted one-time effort to be undertaken in which all the loose ends of the previous efforts are tied together and the resultant system is subjected to a meaningful test cycle. Thus the decision to proceed or not to proceed to engineering development of the system can be made on relatively safe grounds.

If it has been established that feasibility is essential to a successful equipment development program, it follows that a large government agency attempting to mount such a program must have in its organization a permanent and competent group of scientists and engineers working in the exploratory development area. This group must be continuously supported with adequate funding and physical facilities. In return, the group is expected to serve as a center of technical expertise, fully qualified to provide the feasibility determination and to generate new feasible techniques capable of being developed into operational instruments.

It also follows that there must be a second group of competent engineers knowledgeable in their technical area and dedicated to the latter-stage engineering development task. They, too, must be provided with the resources required for their special task. As an

alternative, the original investigators can be allowed to pursue their endeavors into the advanced stages of development, but because they are usually not well versed in the administrative details of those stages the exploratory development effort is diluted.

Normally, a hand-off of development responsibility to the engineering development group occurs with the declaration of feasibility. The original investigators now assume the less active role of technical consultants. The need for close communication between the two groups is obvious, as is the need that each group appreciate the special competences as well as the differing goals and missions of the other. In the absence of such an understanding, there is a real danger that the final development will fail to live up to earlier promises.

requirements statement

The third vital element, the requirements statement, is the document which provides a recognizable shape to a development. Let us assume that the user must add to his operational capability a new measuring technique which has been shown to be feasible. The user must make the first move. He may prepare a formal requirements statement, which in the Air Force is called a Required Operational Capability (ROC), or he may express his need less formally as an immediate operational requirement or a test-range instrumentation requirement. The requirements statement expresses the deficiency that exists in the user's measurement capability, explains why this deficiency must be corrected, spells out the specific performance characteristics of the equipment needed and the time period by which it must be available, and, finally, discusses the known feasible approaches, based on knowledge acquired from the developer. It is staffed throughout the user's command and coordinated with the developer to insure consistency with the state of the art. The developer assists the user to keep the requirement within reasonable bounds. Accuracies are often readjusted to what has been obtainable experimentally. Measurement ranges are similarly treated. Time phasing is sometimes introduced, the user being advised to settle for less ambitious goals initially and to wait for further advances in the state of the art before demanding the ultimate in performance. The investigators also estimate the probable development cost and the time required for completion.

This coordination is a healthy process and usually results in a reasonable requirements document. Once finalized, the document goes forward to Headquarters USAF for approval. If the user's justification is strong enough, a directive is issued calling for a development plan to be prepared by the activity responsible for engineering development.

development plan

The development plan, the fourth essential element in the process, is the blueprint for the latter stages of development, covering all aspects—performance, technical approach, management, documentation, test, resources, production quantities, milestones, etc. When it is coordinated with the user and the original investigators and is approved by Headquarters USAF, funds are allocated, and the full-scale engineering development is begun. Specifications are prepared, a contract is let, preproduction models or first articles are designed and fabricated for test. The preproduction models are tested, ideally in conjunction with the original investigators, against the specified criteria. The user is provided with models for his own test. Specification adjustments and design refinements are made on the basis of the test results, and a firm configuration is established. The production "go ahead" is then given. Finally, the resultant items enter the inventory in accordance with the established milestones.

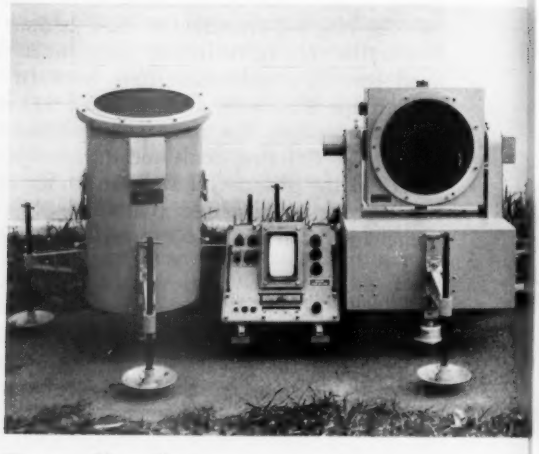
The production release effectively terminates the development process. The new equipment coming off the assembly line should be functionally suitable and highly reliable. This should be the case if the development plan was carefully prepared and coordinated and first article testing and documentation were realistic and thorough.

The latter stages of development are very

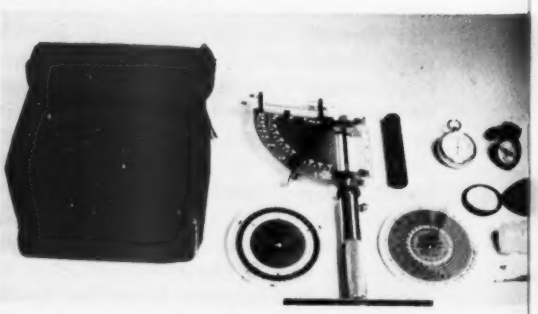
Continued on page 26

Products of Weather Equipment Research

The Air Force has strongly supported exploratory development in the area of meteorological equipment. The investment has returned substantial dividends in experience and knowledge accrued and in operational systems.

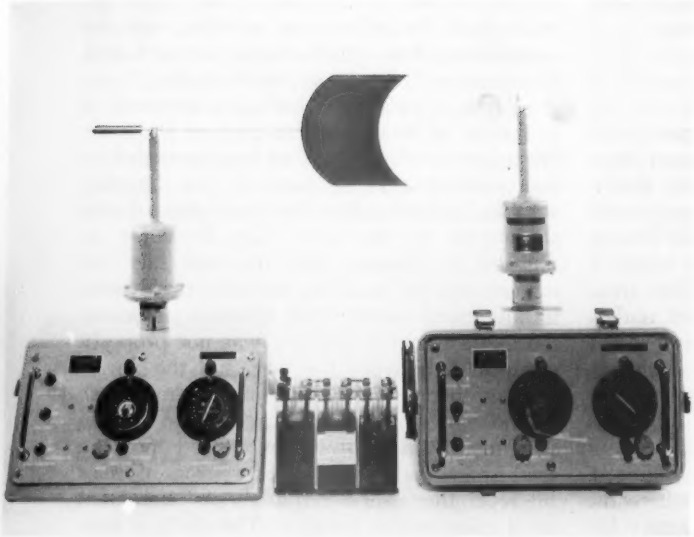


The portable ceilometer, consisting of detector, indicator, and rotating projector, measures cloud height.



The 5-lb weather kit enables AWS teams or native forces to make surface observations without electricity.

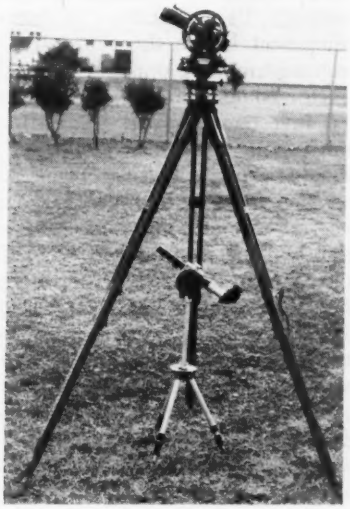
The C-band (5 cm wavelength) meteorological radar set detects precipitation echoes out to 200 nautical miles. Its range normalization, isoecho circuits, and direct view storage tube make it a versatile weather tool.



The wind measuring set designed for field operations (mast not shown) accommodates as many as four remote indicators located 10,000 feet away from the wind speed and direction sensors.

The manual, battery-operated meteorological set, carried at the observer's hip and supported by a shoulder strap, provides precise measurements of wind, temperature, dew point, and pressure.

The three-pound miniature theodolite, designed for use by Special Weather Teams, is dwarfed (albeit with legs retracted) by the standard theodolite.



essential steps, requiring meticulous planning and execution. They are well-documented steps for which instructions and directives abound. They tend to be highly visible, and the coupling with the user is very tight. Therefore, exhaustive treatment of these stages does not appear warranted in this discussion.

THE philosophy I have presented here is a distillation of my impressions from many years of effort in the exploratory development phase of meteorological equipment. Since the precepts set forth were not always followed in the developments with which I have been associated, numerous problem areas were encountered which in retrospect underscore the essentiality of the key elements I have identified. A few examples, described in general terms, will illustrate the point.

The engineering development phase is straightforward and predictable if the essential steps leading up to that stage are followed. I have witnessed several instances where the latter stages of development went astray because insufficient attention had been given to the feasibility concept. In these situations, the engineering development activity made the administrative decision that feasibility had been established, despite evidence to the contrary. User eagerness to initiate final development led to the calculated risk that an engineering development or production contractor could complete the work of the original investigators. Lest this article appear to single out a specific group for criticism, I should explain that in some instances the original investigators also had the final engineering responsibility. In other cases, where they did not, they concurred in the feasibility decision. In any event, the problem lay in the fact that in applying the rules for contractor selection inadequate consideration was given to the need for scientific depth on the part of would-be contractors, in addition to engineering and production skills. The bidders, in turn, failed to realize or appreciate the fact that some residual exploratory development was involved.

In almost every case of this type, serious problems were encountered. A time delay of one or two years in delivery of equipment was common. A less frequent but more severe penalty was the delivery of equipment that was of marginal quality and of very little use in the field. On at least one occasion, outright cancellation of the contract was required, and the user received no equipment at all.

Another situation which has occurred is a change of heart on the part of the user towards a development that has proceeded to the demonstration of feasibiliy, or beyond, with the understanding that the technique was of interest to the user. The developer is shocked to discover that the user has lost interest and the work has no further relevance to the user's needs. This situation indicates that a meaningful dialogue was not properly established between the user and the developer or that it was not properly maintained. In those fortunately rare cases where the development was allowed to proceed into the final stages prior to the expression of noninterest, the user's requirements statement must come under scrutiny. The obvious conclusion is that the requirement was at best marginal.

On occasion, exploratory development is initiated on the basis of an investigator's personal preference, with no real attempt to relate it to user needs or capabilities. It is not surprising that the user would express little or no interest in the technique being investigated—unless, of course, the investigator's choice happened to fit the user's needs. In a well-conducted program, in which the user/developer dialogue is well established and the technical objectives are truly relevant, this seldom occurs.

In contrast to the negative aspects of these cases, many new items of equipment in the meteorology field, for example, are being used routinely and satisfactorily today for operational measurements of atmospheric parameters precisely because the essential development steps were carefully followed. The meteorological rocket systems in use at the various test ranges are perhaps an outstanding example. The user/developer dialogue there

has been exceptionally close, and the results have shown the value of such careful coordination. The rocket sounding system results from a project in which the original investigators also carried out the final engineering aspects of the development. Similar examples can be cited for meteorological equipments in which the responsibility for development was handed off by the original investigators to an engineering development group.

Developers and users in other technical areas within the Air Force will recognize the general types of situation described here and could easily add their own specific details. They would undoubtedly agree that successful equipment developments do not just happen and that a carefully planned program is mandatory. I trust they share my conviction that a farsighted exploratory development program, based on relevant technical objectives and scrupulously adhering to the feasibility concept, will lead to a successful final development—if the user carefully prepares the requirements document and if the resultant development plan is faithful to the user's requirements and to the experimental findings which led to the declaration of feasibility.

Air Force Cambridge Research Laboratories

Notes

1. For an interesting discussion of the foresight which attends the scientist's thought processes, see Chapter 9 of *Science and Government* by C. P. Snow, which was separately published in *Science and Technology* magazine, January 1969.

2. For a strong non-Air Force point of view on this subject, see *Aviation Week and Space Technology*, February 17, 1969. In the feature editorial, Robert Hotz states that the Department of Defense "total package" concept has foundered or is in deep trouble in several instances because of the assumption that no state-of-the-art advances were called for and thus 10-year costing was possible. I am not in a position to refute or support Mr. Hotz's claim, but I believe his editorial is relevant to the need for establishing feasibility prior to firming up other parameters.

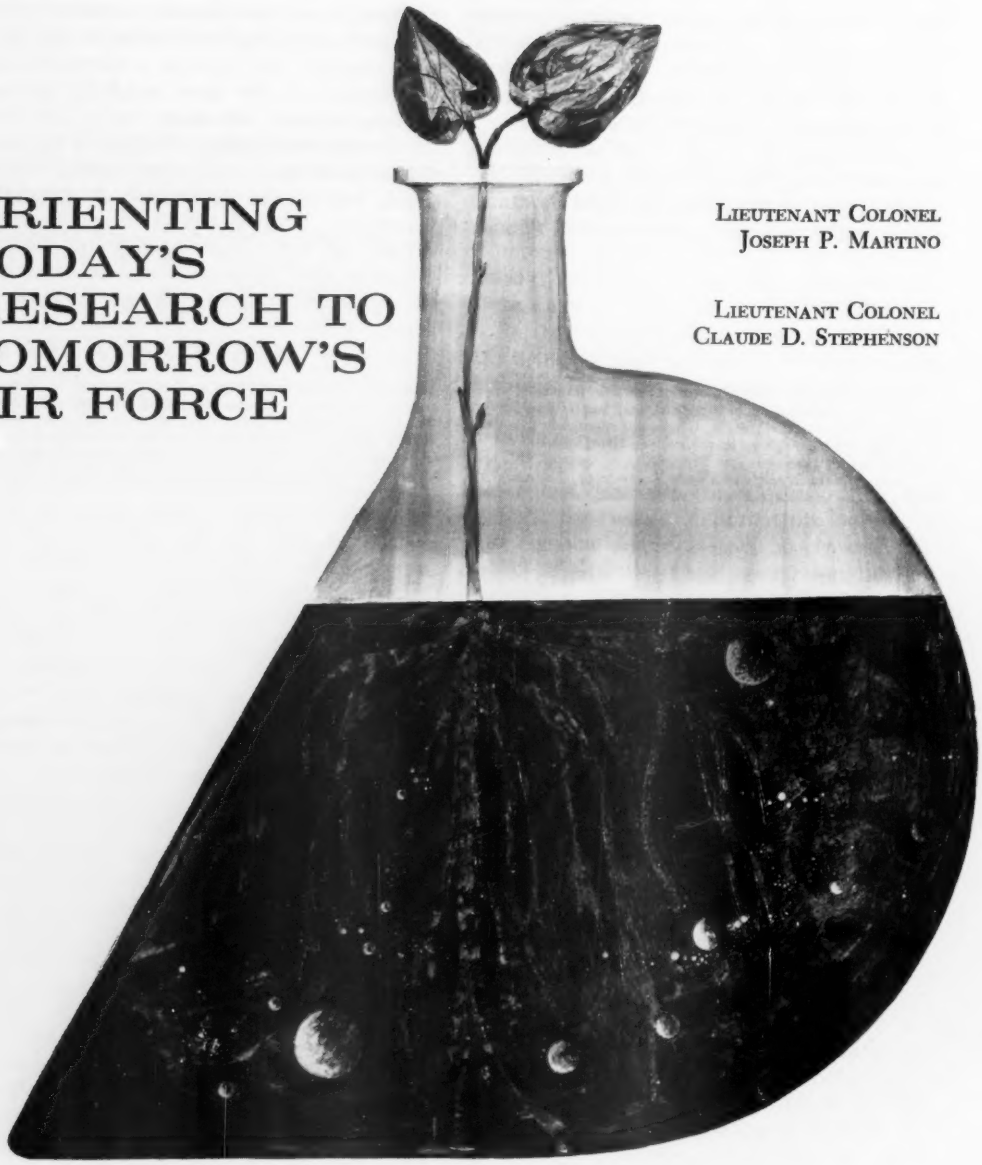
The photographs of fog-seeding operations referred to on page 47 of Major Thomas A. Studer's article "Weather Modification in Support of Military Operations" in our September-October issue were deleted intentionally; the same photographs appeared in similar context in the July 1969 issue of *Aerospace Safety*. Unfortunately, we failed to incorporate the necessary text revisions into the pageproofs.

The Editor

ORIENTING TODAY'S RESEARCH TO TOMORROW'S AIR FORCE

LIEUTENANT COLONEL
JOSEPH P. MARTINO

LIEUTENANT COLONEL
CLAUDE D. STEPHENSON



IT IS a truism that the Air Force of tomorrow will be built on the foundation of the research of today. Research, of course, does not produce hardware. It is not even primarily aimed at solving problems. The primary goal of research is to obtain greater understanding of the universe in which we must live and work. While there are some spectacular examples of individual research having a major impact on technology, as did the laser, such examples are actually few and far between. Over 99 percent of all the results of a research program are like the tiles in a mosaic: individually they are not very meaningful; it is only when they are arranged in the proper pattern that a picture emerges. While the spectacular results are the ones that claim attention, over the long run it is the collection of many, many isolated results into an overall pattern which permits major advances. These many individual results produce a knowledge base on which technology can build to produce solutions to problems.

This situation is not always fully appreciated. There are proposals for the management of research that would essentially restrict it to the investigation of matters connected with a previously chosen technological approach to solving some problem. These proposals fail to recognize that a fully developed knowledge base might show that a particular approach is inherently infeasible and not worth investment of any work, or might show that another approach is actually better. Thus the goal of research is to build a knowledge base to support the developmental problems of the future.

However, the Air Force of the future may also be a captive of the research done today. Today's research may provide knowledge in areas that turn out to be of little importance or fail to provide knowledge in areas that turn out to be of considerable importance. To the extent that this happens, the Air Force of the future will be constrained unnecessarily. It may find itself equipped with hardware that is more expensive, or less effective, than that which could have been built on a better-chosen knowledge base. Thus the future Air Force will be determined by today's research: it may be captive of a poorly chosen research program or beneficiary of well-chosen investigation.

Basic research programs should be relevant to Air Force needs and problems of tomorrow. The notion of relevant programs introduces the concept of mission-oriented basic research. Here selection of research programs is based on their anticipated support for the future requirements of the Air Force rather than as research done for the purpose of acquiring new knowledge per se. The goal of mission-oriented basic research is to provide a more relevant knowledge base to support future military technology.

But even so, how can the research manager be sure that his research program is the proper one? How can he be certain, ahead of time, that he is picking the winners and avoiding the losers? How can he ensure that his foresight, like his hindsight, will be 20-20? The answer is quite simple: he can't. In his work he is faced with two uncertainties: uncertainty about what his research program

will actually achieve and uncertainty about how valuable something will be even if it is achieved. In the face of these uncertainties, he can never be sure he is making the right choices.

However, his problem can be viewed from another standpoint. He is forced to make choices anyway. He is always faced with resource constraints. There are never enough manpower spaces available to do everything he would like to do; there is never enough money available to buy all the equipment and supplies he feels he needs; there are only so many hours per day available on critical facilities. Since he cannot do everything he would like to do, he must decide which projects will get extra manpower and which will not; which items of equipment will be procured this year; which programs will have priority on critical facilities. His goal is to make these choices as intelligently as possible. His decisions must be made in the light of the possible consequences of each choice and the impact of those consequences on the Air Force of the future.

One of the first considerations of the Air Force research manager is: What is the rest of the research community doing? The Air Force funds something less than ten percent of all basic research being done in the United States. Thus most of the research which the Air Force eventually applies will in fact be funded by some other source. But there will always be specialized facilities which have no application outside the Air Force—areas of science which are of little interest to the other services or to non-Defense industry; programs which are too expensive for a university. So the Air Force research manager must look for those things which will not be done at all, or will be done too slowly, unless the Air Force supports them through contract or does them in-house.

Even within these more limited areas, of course, there is still much more to be done than will ever be possible within the resources available to the Air Force research manager. He still must make intelligent choices. To make these choices, the research manager must have decision information which reduces the uncertainties about his program. Uncertainties about what will be achieved can never be eliminated, but they can be reduced by well-designed experiments. This aspect of research management is beyond the scope of this article and will not be pursued further here. Uncertainty about the value of results if they are achieved can also be reduced, to the extent that the research manager has knowledge about the environment in which the future Air Force will operate and the problems it will face in that environment. The remainder of this article will be devoted to describing approaches to minimizing the research manager's uncertainty about the environment of the future and the problems it will pose for the Air Force.

research analysis

How can the research manager choose among those research projects which he sees as being of good scientific quality, not likely to be done unless his organization does them, and of some apparent interest to the Air Force? Undoubtedly even this limited set of possible research projects will exceed one or more of the constraints on his total amount of resources. He needs to reduce in some way the uncertainty about the value of the proposed research projects, if they are successful.

A planning tool known as research analysis can be helpful to the research manager in obtaining information about the potential value of proposed research efforts. Research analysis is usually considered to involve two types of effort. Both draw heavily on the methods of operations research. However, the emphasis in one is completely reversed in the other. These two types of effort are research applications and research opportunities.

The first, research applications, addresses the question of the utility of a specific piece of completed research. Given a scientific result which makes a statement about some natural phenomenon, this statement must be translated into terms meaningful to the engineer and to the operational user. A research applications study is intended to make this translation. It takes the quantitative scientific re-

sult, as a statement about some phenomenon, and converts it into a quantitative statement about some engineering parameter or into a quantitative statement about some operational capability. Thus the engineer or the operational user can directly compare the potential contribution of this scientific result with what he currently has available and determine the value of exploiting this result. Clearly, the emphasis of research applications is toward the user of research rather than the research planner, and it will not be discussed further in this article.

The other type of effort used in research analysis, namely research opportunities, is concerned directly with the problems of the research planner. It is intended to start with a statement of technological or operational deficiency and derive precise and quantitative statements about scientific research needed to overcome these deficiencies. That is, it is intended to tell the research manager which phenomena should be investigated in order to provide the knowledge base from which a solution to the stated deficiencies can ultimately be drawn. Three points should be made about this type of analysis.

First, it is important to recognize that there is more to the Air Force than combat hardware. The Air Force consists also of people and procedures. Thus the deficiencies which provide the starting point for research opportunities studies must include "people problems" and "software problems." Clearly, there is little value in conducting research which leads to excellent hardware if that hardware is rendered ineffective by problems arising from the choice of people to man it or procedures to control and integrate its operation. Also of importance, but often overlooked, are items of noncombat hardware, such as communications and logistics. Deficiencies in these areas are also important starting points for research opportunities studies.

Second, it must be recognized that a research opportunities study is not intended to indicate, ahead of time, the one area of scientific endeavor guaranteed to lead to a solution of the original deficiency. On the contrary, it is intended to indicate all those areas which appear to have a reasonable likelihood of contributing to the solution. The objective is not to lead the research manager to the one specific area which should be investigated but rather to make sure that he does not overlook some area which might make a major contribution. Clearly, since a multiplicity of scientific areas will be indicated as sources of solutions to specific deficiencies, one of the criteria the research manager will use in formulating his program is to select research areas shown to lead to the solution of a multiplicity of deficiencies. In this way his program will be less sensitive to errors in the analysis.

Third, to re-emphasize what has been stated earlier, the research cannot be based on attempting to solve today's problems. Thus the deficiencies which provide the starting point for research analysis must be those projected for some time in the future in order that the research program can be completed in time to be useful. How are these future deficiencies to be identified? This is the subject of the next section.

systems analysis

Systems analysis is a tool which, when properly applied, can aid the research manager in the task of selecting areas of technological deficiency. Systems analysis, while not new, has enjoyed widespread popularity within the Defense community as a tool for making selections between competing weapon systems and operational concepts. However, many current applications of the technique use the cost-effectiveness criterion as a discriminant for selection of a specific system for the weapon system inventory. This approach, while important and satisfactory for the systems selection process, does not provide much valid decision information for the research planner.

In recent years a variant of systems analysis has been applied to develop the needed information for research planning. Here the technique is used as a vehicle to identify technological deficiencies rather than for selection of weapon systems. While cost-effectiveness considerations are employed and preferred choices are illuminated, these do not

form the major thrust of the studies, which is to identify areas of potentially high payoff for technological improvements. As noted, such decision information is required to properly identify research opportunities. It is information about the possible future technological deficiencies which the planner needs to lay out a good research program.

How does one go about using the tools of systems analysis to identify technological deficiencies? How can this tool be of use to the research manager? Can all systems analysis studies be used in this way to develop decision information for the research manager?

Most systems analysis studies can be structured at the outset to give information about technological deficiencies, and many completed studies can be updated and continued to give this type of information. The key in each case is to perform a sensitivity analysis of the various components used in the overall technical solution to the problem. In a sensitivity analysis the technological parameters of various components are varied systematically to determine the effect on total system performance. The idea is to isolate those areas where modest improvements result in a high payoff in performance. While this type of analysis is frequently used in the design of specific hardware, it also applies to large-scale systems that contain elements using dissimilar components. An example of one is a ballistic missile defense system.

Let us assume that the goal of a ballistic missile defense system is to intercept the incoming warhead at a point as early in the trajectory as possible. There is an obvious tradeoff between the interceptor and the sensor system. That is to say, with a better sensor one could tolerate some reduced interceptor performance and still achieve the desired goal. Likewise, increased interceptor performance would permit a reduction in the required sensor performance. Or, if an increase in the intercept altitude were desired, one could determine whether it could be obtained more easily and cheaply through improving interceptors or sensors. By using these approaches, one could arrive at specific technological tradeoffs, such as relating the specific impulse of the interceptor propellant to the power output or receiver sensitivity of the sensor radar.

This approach, of course, has historically been used to obtain near-term information for the design of a specific system. Its use as a research planning tool, however, is relatively new. In this application, future known or assumed military mission tasks are studied and possible technological approaches are postulated for their solution. These approaches can usually be stated in broad terms. Since the analysis is not intended to pick out a uniquely best solution, exact projection of costs and performance is not required. The only thing necessary is a consistent analysis that produces a base line of cost and performance data for sensitivity analysis. The purpose is to identify areas of possible technological deficiency, not to obtain the cost and performance data that would enable a choice of the best possible system. Once a base line has been obtained, it is possible to vary the estimates of performance of the various possible system components to illuminate technological problem areas. It is also possible to consider components or specific techniques which appear totally incapable of reaching adequate levels of performance. The technological barriers identified in this way, as well as the estimated payoffs from increases in component performance, then serve as inputs to a research analysis study.

The results of a systems analysis, however, are of value to the research planner only if they address tomorrow's problems. The mission tasks selected for study by systems analysis must be relevant to the needs of the environment in which the Air Force of the future will operate. It is not sufficient to assume that because we have always done a certain thing we will continue to do it. Nor is it sufficient to assume that since we have always had weapons of a certain type we will continue to use them. The mission tasks selected for study must be based on sound estimates of the requirements of the time period 10 to 20 years into the future. The tools and techniques for obtaining these estimates are discussed in the next section.

mission analysis

Mission analysis, or mission identification as it is sometimes called, is one of the newest techniques available to the decision-maker. Man has long studied the various phenomena with which he is confronted, at first learning to explain and then to predict future occurrences of the phenomena. In recent years scholars have approached the realm of human affairs with the same inquiring analysis that has characterized studies of the natural sciences. While still in its elementary stages, some progress is being made, and new insights and techniques are being developed which have utility to the planner and the decision-maker.

Mission analysis involves two steps: the first is to develop a range of potential conflicts for the future time period, and the second is to transform these conflicts into mission tasks useful for their solution. Again there is nothing new about this approach; the improvements are in the manner of doing the study and how the information is developed.

The reliability and credibility of the list of possible mission tasks used for planning are strongly related to the reliability and credibility of the future conflict picture as it is understood at the present time. To be sure, some improvements are needed in the techniques for transforming conflict data into mission tasks, but the key to successful mission identification continues to be in developing better conflict information.

One approach is to postulate a spectrum of conflicts. In the crudest form, it is assumed that all the conflicts are equally probable. In some studies these various forms of conflict are developed into scenarios in order to give the information a feel of plausibility. Probability estimates or rankings can also be developed to ease the decision-maker's task in selecting from this array of potential conflicts. However, it should be clear that the validity of the selected spectrum of conflicts has direct bearing on the validity of the derived array of mission tasks. These, in turn, have a bearing on the relevance of the technological deficiencies that are derived from the systems analysis studies using these various mission tasks as input data.

Recent investigations at some leading universities indicate that systematic studies can be made of the international sociopolitical environment to identify areas of potential conflict. Many of these studies apply quantitative techniques, which have the advantage that they can be duplicated and understood by other investigators. While the validity and reliability of these quantitative techniques remain to be proved, the potential does exist for the capability of developing improved conflict data.

One observation is important at this time. While planning for the long range (which is characteristic of planning for research) does require valid, reliable, and relevant estimates of future conflict, it does not follow that the group which does the planning must also develop the conflict data. The important factor is that valid estimates of future conflict patterns are essential to developing a plausible mission task structure necessary for research planning. The essential factor for the research manager is to have assurance that the input to the planning process is valid. For research planning this means that the mission task structure used for developing technological needs does represent a valid estimate of the future.

What has been described here is a process or method whereby research managers can get better information on which to base their decisions for the allocation of today's resources to solve tomorrow's problems. Many of the techniques are being used today, while others require further refinements before they are a part of the everyday tool kit of the research manager.

interactions among methods

As is indicated by these descriptions, the areas of research analysis, systems analysis, and mission analysis are each important in its own right. A single laboratory, for instance, may carry out a program of research analysis based on statements of deficiencies obtained from higher headquarters or other organizations. An organization concerned with systems development may well carry out a program of

systems analysis utilizing information about missions and environments obtained from various service or joint planning documents. Mission analysis could well be carried out by major operating commands interested in preparing long-range plans or by Hq USAF. In fact, there is a considerable amount of effort in each of the three areas in various Air Force organizations. Nevertheless, there are distinct advantages to carrying out all three activities in the same organization.

One important consideration is the ability to judge the quality of work done elsewhere. Even if an organization does both systems analysis and research analysis, for instance, it is unlikely to do all the systems analysis required as input to its research analysis efforts. Even if it had a large enough staff, some of its work would inevitably duplicate work done elsewhere. Hence any organization will normally attempt to take advantage of information produced elsewhere. However, the presence of an in-house capability in the same field, with adequate depth of experience gained through conduct of the same kind of work, will allow the organization to identify the limitations and strengths of studies conducted elsewhere. These studies can then be used with more confidence as information sources.

A second advantage, somewhat less obvious, is the ability to modify studies done elsewhere, to make them more compatible with the internal requirements of the organization. A study done elsewhere for some specific purpose may not be exactly suitable for the purpose intended by the organization planning to use it. An in-house capability to do the same type of study will enable the organization to make the desired modifications in initial assumptions, mathematical models, or data sources quickly and easily. Otherwise, the organization will have no alternatives except to use or reject an externally produced study as is.

A final advantage, although somewhat subtle, is extremely important—perhaps more important than the two already listed. This arises from the synergistic interaction of the three activities when carried out in the same organization. The results of a research analysis study, for instance, may completely alter the initial assumptions of the systems analysis study which preceded it by uncovering a previously unsuspected capability. Similarly, a systems analysis study may produce results which significantly alter the inputs to a preceding mission analysis study by showing a previously unsuspected impact of an offense-defense interaction. When these feedback effects are encountered, it is very valuable to have all three capabilities available in-house so that the sequence of studies can be iterated until a stable solution is reached. It is virtually impossible to obtain this synergistic effect when an organization produces only one type of study and uses as inputs studies done by other organizations for their own purposes.

Office of Research Analyses

The Office of Research Analyses (ORA) is an element of the Office of Aerospace Research (OAR). Its assigned mission is to develop and maintain an in-house capability to conduct all three of these types of studies. Much of the OAR effort in these areas is carried out by ORA, which also serves as a source of planning information for the other elements of OAR.

The Office of Research Analyses has at least one effort under way at all times in each of the areas of research analysis, systems analysis, and mission analysis. Many of these studies are done in response to requests from outside organizations. Others are generated internally as a result of a need uncovered by one or more members of the staff. Results of all studies are available to interested user organizations. However, the emphasis in all studies, whether done in response to an outside request or internally generated, is on providing planning information and guidance to the various OAR research planning staffs and research managers.

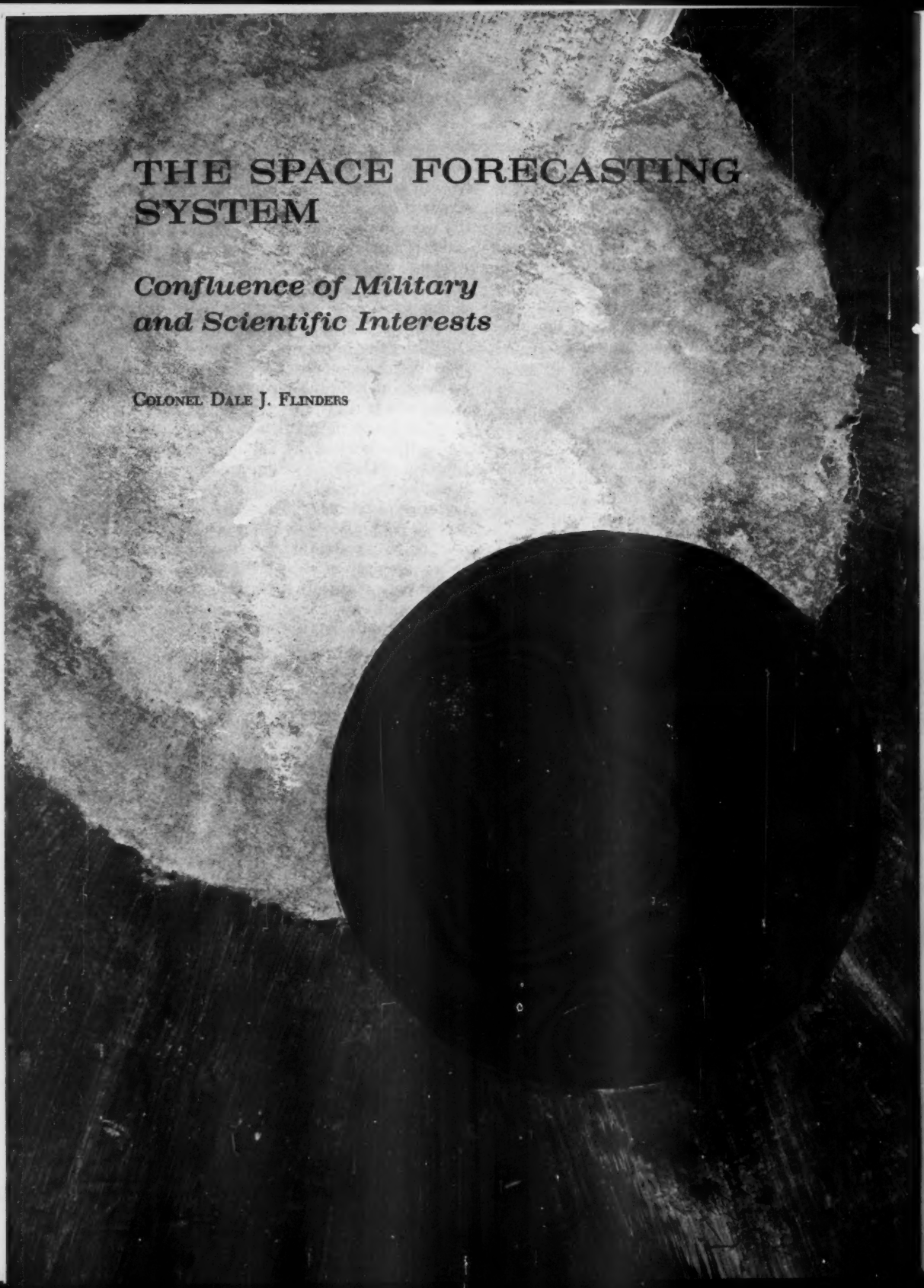
One important product of the ORA research program is new methodology for conducting each of the types of studies. Advances in analysis methods, new and more effective techniques, and more accurate or more general models are major outputs of the ORA program.

Thus ORA provides a significant input to the planning activities of the OAR research manager, both through conducting studies whose results are useful to him and by developing new methodology for research analysis, systems analysis, and mission analysis. Since all these activities are carried out in the same organization, OAR benefits from the availability of an in-house capability in each activity and from the synergistic interactions of the three activities carried out simultaneously.

RESEARCH analysis, systems analysis, and mission analysis can provide the research manager with information about which areas of scientific endeavor are likely to have a high payoff in terms of providing the knowledge base to solve the problems of the Air Force of tomorrow. This information may still not be sufficient, however, to enable him to make decisions about allocating his resources. It may happen that an area of research which appears to be important is not yet ripe for activity. Before the needed research can be carried out, there may be a requirement for instrumentation advances, additional knowledge from another field, or some other step. Or there may be only a limited number of scientists of the required specialty available in the country. Or the work might require an expensive and specialized facility, identical to one already in existence elsewhere. A whole host of considerations must be taken into account before deciding to carry out research which has been shown to be of value to the future Air Force.

Thus the information obtained from the cycle of mission analysis, systems analysis, and research analysis is simply one input among many which the research planner must consider. It by no means tells the whole story or forces the decision in a certain direction. The function of this type of input is to reduce the uncertainty about the value of research in various areas of science. The research planner must still consider other factors. Nevertheless, this reduction of uncertainty is a very valuable asset. When properly used by the research planner, it can help ensure that his research program will expand, rather than constrain, the possibilities open to the Air Force of tomorrow.

Office of Research Analyses (OAR)



THE SPACE FORECASTING SYSTEM

Confluence of Military and Scientific Interests

COLONEL DALE J. FLINDERS

A LARGE Air Force environmental support capability is being assembled—quietly and with little attention by the military and scientific communities—to be placed in operation on 1 March 1970. The system is designed to predict hours, days, or weeks in advance bursts of radiation and energetic particles from the sun and their effects on the earth's atmosphere.

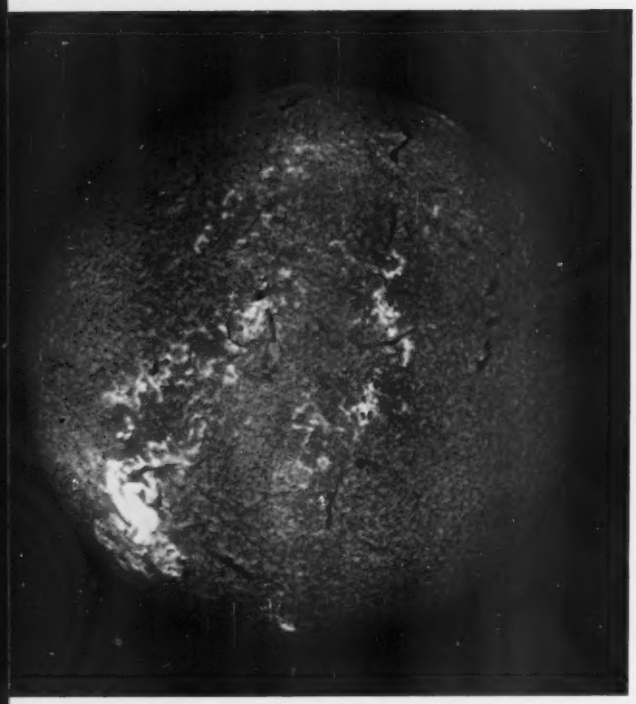
The system draws on most of the environmental sciences, the product of which forms a complex matrix of causal interrelationships involving radio and optical solar astronomy, astrophysics, radio propagation physics, and the physics and chemistry of the upper atmosphere. The observational phenomena with which these scientific studies deal have a direct and sometimes profound influence on a range of Air Force electronic and optical systems. Radars, delicate satellite sensors, radio communications, and navigation systems are all subject to changes in performance due to the occurrence of astrogeophysical events. The events of interest are absorption of radio waves in the polar regions, geomagnetic disturbances, auroral activity, enhanced airglow levels, variations in atmospheric density, and ionospheric fluctuations and inhomogeneities. It is of obvious concern to the Air Force to know well in advance any predictable degradation of its electromagnetic operational systems. All these environmental effects are triggered by one or both of two possible causes: bursts of electromagnetic (EM) radiation and energetic particles from the sun; (2) the "dumping" of particles from the earth's radiation belts into lower levels of the near-earth space environment. A particularly devastating phenomenon is the impinging of high-energy solar protons into the polar cap regions.

If we can predict the magnitude of bursts of particles and EM radiation from the sun, then we can in theory predict some ultimate effects on Air Force operations. This presupposes that we understand the relationship between a particular solar event and a particular terrestrial effect. At present we do not know these relationships except coarsely and qualitatively. Nor do we know with any degree of precision the precursor solar features that trigger the solar burst. The latter calls for a more thorough knowledge of the dynamic processes on the sun; the former calls for an understanding of solar-terrestrial relationships in greater depth. As our knowledge and understanding increase, so will the accuracy of our predictions.

The system that will both accelerate the acquisition of such knowledge and make the predictions is being developed by the Air Force Cambridge Research Laboratories (AFCRL) and the Air Weather Service (AWS). The heart of the system, after it becomes operational, will be the Air Force Global Weather Central complex of Univac 1108 computers at Offutt Air Force Base, Omaha, Nebraska. Linked to these computers is a worldwide net of optical and radio observatories, which will provide real-time inputs from the radio and optically observed sun. Every observable feature of the sun will be monitored by the system, together with changes in these features and their rate of change. In addition, observations from riometers, neutron monitors, magnetometers, and satellites will be fed directly into the network. From these observations the AWS-AFCRL system designers hope to define the conditions that precede a burst of solar energy and the terrestrial effects that follow it.

In the future the system observational net will be expanded to include a more comprehensive array of both solar and terrestrial monitoring stations. But right now, the problems of standardizing data inputs, calibrating the sensing instrumentation, and developing the basic system software are about all that the system designers can cope with. The requirement for precise standardized reporting and the need for uniformly calibrated instrumentation have been the foremost difficulties in bringing the system to fruition; when achieved, they may prove to be the system's major legacy to science.

Technical planning and systems evaluation are done through the Working Group on Space Forecasting, which meets monthly to plan, review schedules, assign priorities, and look into special problems. The Working Group is composed of about twelve AFCRL and



The sun, when photographed in hydrogen alpha, shows filaments or prominences as dark lines because their temperature is cool relative to that of the solar photosphere. The white areas are plages, usually associated with sunspots, which are of primary interest because it is from them that flares originate, emitting explosive bursts of radio energy, photons, and x-radiation.

about six AWS representatives, plus many interested representatives from other government organizations, the composition and number varying from meeting to meeting.

The Working Group was set up early in 1966, but much work on the system had already been completed, and the AWS was already doing routine forecasting. Many of the effects of solar eruptions on the terrestrial environment were known in a general, qualitative sort of way. Since the 1950s the Central Radio Propagation Laboratory, formerly under the National Bureau of Standards but now part of the Environmental Science Services Administration (ESSA), has made regular predictions of geomagnetic disturbances based on bursts of radio energy and optical and magnetic features observed on the sun. In 1964 the Air Weather Service established its Solar Forecast Center, which is now located in the NORAD Cheyenne Mountain complex near Colorado Springs, to provide forecasts relating to Air Force operations based on similar observations. Also, in the early 1960s, several major optical observatories were predicting periods when there would be an absence of solar proton showers. Foremost among the observatories working on the problem was the AFCRL Sacramento Peak Observatory in Sunspot, New Mexico. NASA relied heavily on these predictions in connection with the early Mercury flights.

Many of us first learned of high-energy solar protons in connection with these Mercury flights. We learned that high-energy solar protons were an ionizing hazard to man in space and to electronic systems aboard aircraft. True, but perhaps overdramatized. (The mental picture of an astronaut being bombarded by radiation has an emotional impact and thus is easily remembered.) Air Force interest encompasses this man-in-space problem, but the priority interest is the effects of these protons and associated solar emissions on the terrestrial environment.

The forecasts that the AWS Solar Forecast Center provides its customers—NORAD, SAMSO, and others—are a normal extension of the AWS weather forecasting mission. The center receives regular daily transmissions on the status of the sun from almost a dozen radio and optical telescopes around the world. A primary source of radio data is AFCRL's Sagamore Hill Radio Observatory in Hamilton, Massachusetts, where AFCRL and AWS have for many years continuously monitored the radio spectrum of the sun. The Solar Forecast Center manually inspects the data for any pattern that might foretell eventual terrestrial effects.

In late 1963 AFCRL undertook a large new endeavor, designated the Space Forecasting Program. This program, in which AWS people work side-by-side with AFCRL scientists, can be

Air Force Cambridge Research Laboratories—Air Weather Service Working Group on Space Forecasting

viewed as a special research effort in support of the AWS Solar Forecast Center. Before this program got under way at AFCRL, no one had taken a broad look at all aspects of the forecasting problem in an attempt to tie them together as an entity. Solar, radio, and atmospheric physicists were all doing much research related to forecasting and were contributing useful solutions, but each was considering the problem from the narrow perspective of his own discipline.

By 1966, work was in progress at three loosely coordinated levels. There was the operational AWS Solar Forecast Center, providing daily predictions to its Air Force customers. There was the Space Forecasting Program at AFCRL, doing studies and research directly related to the work of the center. And there were the scientists conducting generalized research on environmental phenomena. It became apparent that work at these three levels had to be brought together in a closer working relationship. One reason making this need immediate was that the AWS Solar Forecast Center had embarked on a program for markedly upgrading its observational network and required a closer working relationship with those involved in the Space Forecasting Program at AFCRL. And the Space Forecasting Program people had come to realize the need for

By using a coronagraph attached to a telescope (which produces an artificial eclipse of the sun), Air Force Sacramento Peak Observatory photographs solar prominences on the sun's limb. Spectacular but benign, the prominences emit none of the high-energy particles and radiation of flares.

The main dome of Sac Peak . . . its central spar, with telescopes and associated instruments trained spaceward. Sac Peak's forecasts of proton showers have been routinely submitted to Environmental Science Services Administration (ESSA) since the first manned Mercury mission. Relayed to National Aeronautics and Space Administration (NASA), they enter into the scheduling of space ventures by warning of potentially dangerous periods of solar activities.

closer participation by specialists in a diversity of fields—energetic particles, ionospheric variability, the earth's magnetosphere, atmospheric density and so on.

Through the Space Forecasting Working Group, an interface among the three levels was effected. The Working Group had two primary tasks, the first immediate and specific, the other more general. The first was to review, advise, and work out problems associated with the updating of the observational network of stations and the basic reduction of these data for the AWS Solar Forecast Center. These particular problems have occupied most of the attention of the Working Group to date. The other task was to examine all available scientific knowledge that might be applied to improve the accuracy of predictions and extend the warning time.

Considerations of accuracy and warning time lead to the physics of the problem. This background will help make a description of the system and some of the problems of bringing it into being more meaningful. All bursts of electromagnetic energy and high-energy particles from the sun are associated with solar flares, and solar flares (with a few rare exceptions) are associated with sunspots. But not all sunspots produce flares, and not all flares produce disruptive solar bursts. Of three basic prediction problems, the first is that of predicting whether a given flare, once it has been observed optically and by radio, will emit an intense flux of radiation and/or particles.

The second problem is that of predicting effects on the earth's environment—magnetic

storms, auroras, ionospheric disturbances, and so on. The effects and their onset vary because the ratios of intensities of the different emissions in the solar burst vary markedly from flare to flare. High-energy protons reach the earth from a few minutes to a few hours after the flare peak. Other protons are strung out in time (and space), and the maximum flux of protons is usually observed several hours after the first measurable terrestrial effect. Low-energy particles emitted at the same time travel much slower. Their effects are observed two or three days later in the form of magnetic storms and ionospheric disturbances worldwide. These electrons modulate the earth's magnetosphere, causing magnetic storms. The problem is that of predicting not only whether a solar flare will produce a terrestrial effect but also the kind of effect, its onset, and its duration.

The first two prediction problems are concerned only with effects, if any, once a flare is observed. This means that predictions are limited to about two days in advance. The third problem, that of forecasting the occurrence of the flares, can only be solved by monitoring the visible features of the sun and associated magnetic configurations. However, this would yield longer-range forecasts. We do not yet know, except in a general way, the characteristics on the visible surface which portend a flare that will produce disruptive energy. What we can predict at the present time is that under certain limited conditions no disruptive event is likely to occur during the next three to five days.

Long-term prediction has been primarily the concern of the optical astronomer, while short-term prediction has been largely in the province of the radio astronomer. Radio astronomers in 1968 reported some extremely favorable results in making forecasts up to about two hours, based on solar radio emissions in the centimeter range. AFCRL radio astronomers can now say with about 80 percent assurance that, if a certain characteristic radio spectral pattern is observed, it heralds the arrival of high-energy protons in the earth's atmosphere.

In addition to optical observations and centimeter radio observations, there is another promising instrument that may provide a key parameter for prediction. This instrument is a radio telescope that observes solar emissions in the millimeter range (35 GHz). AFCRL operates such a telescope at its site in Waltham, Massachusetts. From millimeter wave emissions it is possible to map temperature profiles just above the visible surface of the sun in the chromosphere. Activity centers on these radio maps of the sun seem to be highly correlated with bursts of radio, X-ray, and particle emissions from the sun.

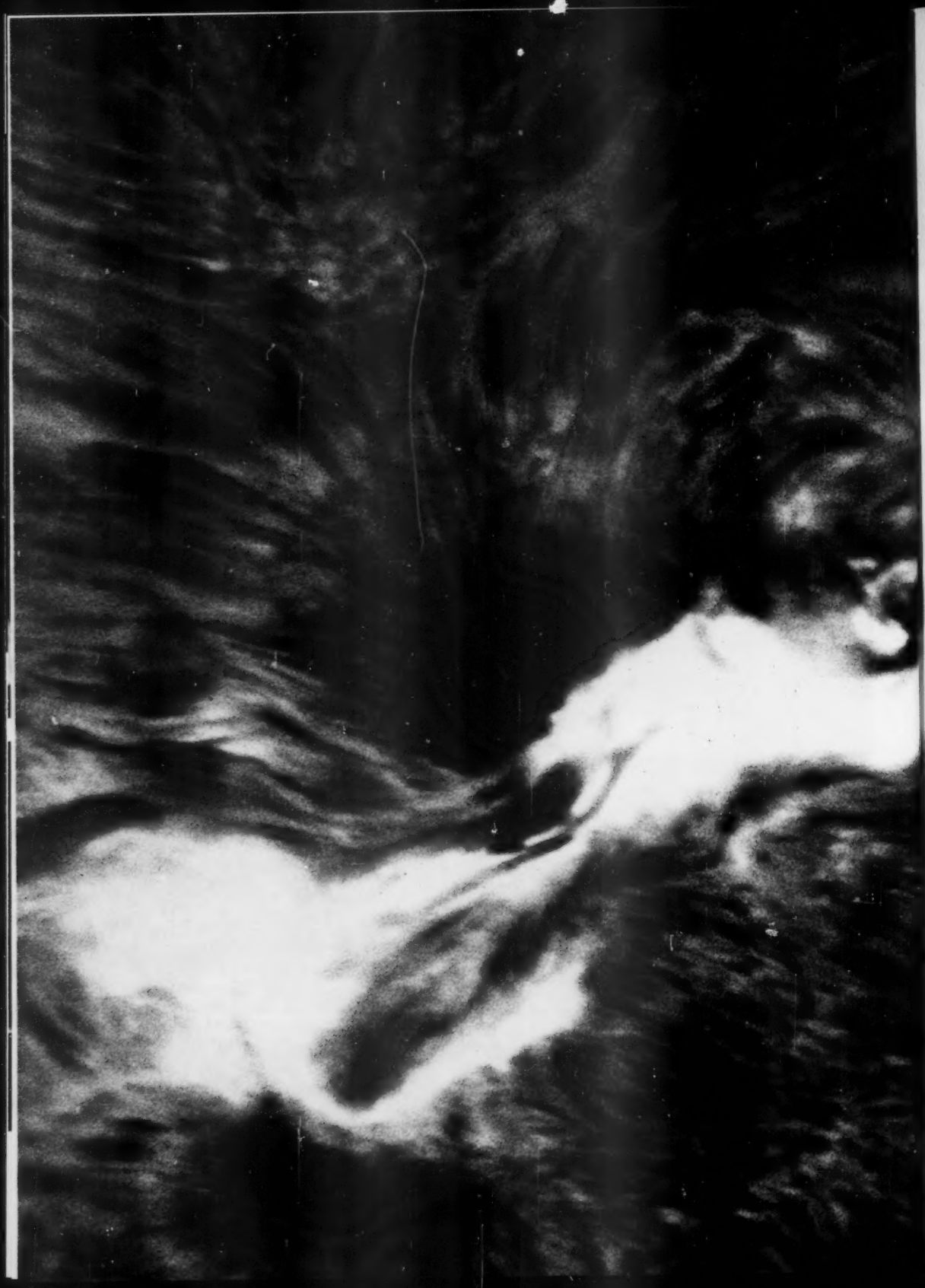
Future plans call for observing the ultraviolet (UV) and soft x-radiation (XUV) emissions from the sun with satellite sensors. Almost no work has been done to date in correlating UV data with proton events, but such observations may prove most valuable. The ultimate system—but not the initial system that will become operational in 1970—will observe all solar emissions across a broad spectrum of radio and optical wavelengths.

The forecasting task appeared to be rather straightforward to those who planned the Space Forecasting Program at AFCRL in 1963. AFCRL decided to concentrate its initial attention on optical rather than radio observations because there appeared to be fewer problems demanding attention in the radio area. Whatever the radio astronomers learned could be melded into the overall system later.

To start, AFCRL scientists opened what they believed to be a treasure chest of available data. Daily records of the visible sun extending back three decades had been accumulated by observatories all over the world. These daily records of sunspot groups and flares, when properly analyzed and subjected to a range of statistical processing techniques, would quickly reveal prediction correlates, so it was believed. There was no corresponding record of solar energy bursts—at least no direct record; but past solar events could be deduced from the recorded history of terrestrial effects, such as geomagnetic disturbances, auroral activity, and so on.

Within a year after work with these historical records began, the AFCRL space forecasting team faced a problem that was to alter drastically the course of its planning. Past

The first evidence of high-energy solar protons in space was gained quite incidentally. NASA's satellite Discoverer 17 carried aboard a photographic emissions plate designed to detect cosmic rays. When recovered and developed, the plate showed a thick mean of ionized tracks. They were determined to have resulted from a burst of high-energy protons from the sun during Discoverer's flight.



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Solar events that affect communications, navigation, and surveillance on earth usually emanate from extremely hot spots, one of which is in upper right of this temperature plot of the sun made at 35 gigahertz (GHz).

Among the dynamic features of this active solar center are the sunspot (dark area) and the striations (formed by magnetic field lines) leading into it. The white area is a solar plage.

AFCRL's high-precision 29-foot mm wave antenna measures solar temperatures for contour plots.

solar records were almost useless from the standpoint of systems planning. Examination revealed gross imprecision, inconsistencies, and multitudinous errors throughout. To solar scientists, the AFCRL analysis proved particularly traumatic, since it revealed gross fallacies in the international historical record and thus placed in question all scientific studies making use of these records.

The reasons for the inconsistencies were not hard to identify. Because each observatory tends to be an isolated scientific island unto itself, reporting procedures and interpretations of observations varied widely. Although the International Astronomical Union sets reporting standards which all members should follow, these standards have undergone frequent revisions over the years. There was often a lag before all observatories adopted them, and some never did. The mathematical conversion of data from one standard to another in order to achieve a common data base was a monumental task. Those working on the space forecasting problem, however, were willing to undertake it.

But in the process of examining the records from all the observatories, another and even more critical source of error was uncovered. This had a serious implication which in 1967 forced the Air Weather Service to reconfigure the entire network structure of the Space Environmental Support System. The optical inputs for the system, as originally conceived, were to come from established solar observatories. But two observatories, both looking at the sun at the same time and both reporting according to the latest international standards, often reported disparate data. Why was this? It was because each tended to calibrate its instruments in its own way or to use distinctive filters that it believed provided the best view of the sun.

As a result, AWS in 1967 decided that its Space Environmental Support System could not rely on data from existing observatories. AWS at that time concluded plans to establish new observatories whose exclusive function would be to provide data for the Space Environmental Support System. These observatories would be located worldwide so as to

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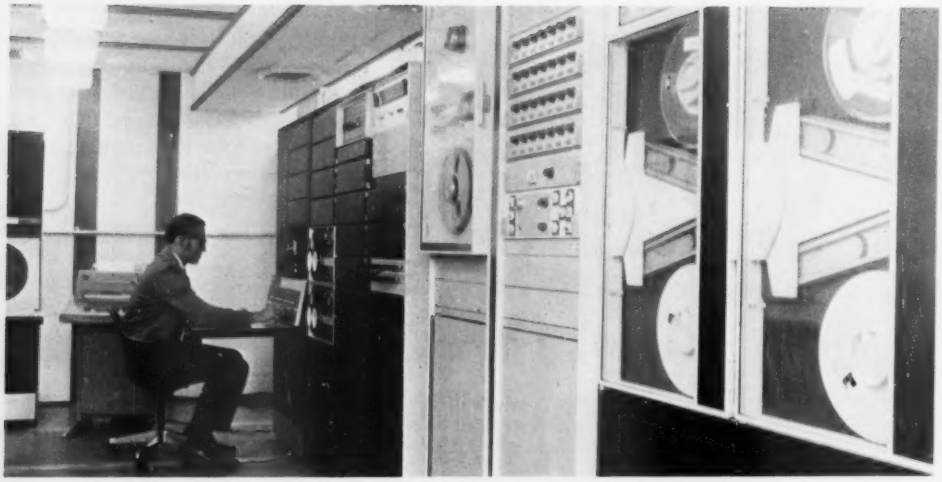

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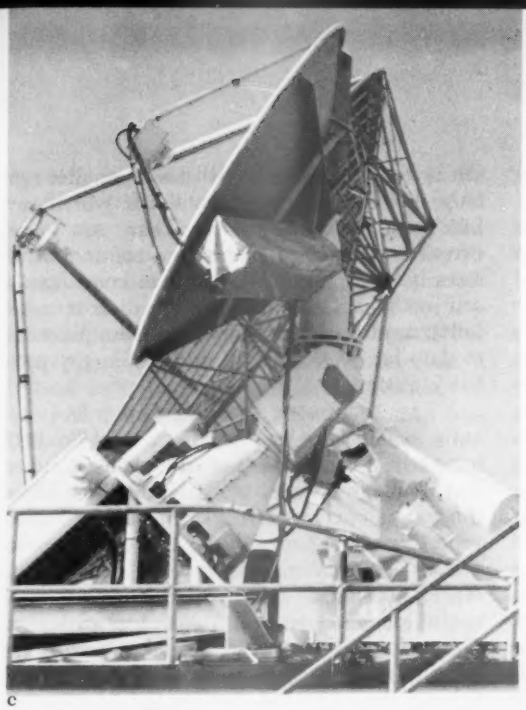

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Sagamore Hill Observatory

AFCRL's Sagamore Hill Observatory at Hamilton, Massachusetts, sends a daily record of solar radio emissions to the AWS Solar Forecast Center in Colorado. Radio events of special interest are teletyped to the Center immediately. The data gathered are first recorded on electronic data-processing equipment (shown in a*) . . . Five telescopes of various ranges are used for continuously monitoring the sun: The 150-foot dish (*b*, foreground), with the 84-foot dish (background). . . . The 8-foot and 28-foot instruments (*c*). . . . And the 3-foot and 8-foot antennas (*d*).*

give continuous coverage of the sun. Sites were selected in Tehran, Puerto Rico, Hawaii, and the Philippines, and others are planned whose locations are not yet determined. Radio telescopes will also be located at three of these sites. The relatively small optical telescopes presently used, with a five- or ten-inch refractor, do not yield consistent data. Therefore, in order to eliminate the last major deficiency in the optical network, a complete reequipment with identical solar telescopes is planned for the early 1970s. A test program is now under way in which the first pair of identical solar patrol telescopes will be set up side-by-side for thorough calibration in order to ensure uniformity.

Another revision in system design occurred in 1965. Initial planning called for the manual analysis and interpretation of the solar and radio telescope observations of the sun. But clues for prediction are often intricate and subtle, requiring careful subjective assignment of weighting factors. More important, the system as it evolved was seen to generate massive amounts of data, the interpretation of which would exceed the capabilities of the most skilled analyst. When all this became apparent, the decision was made to automate the system. Data from the observational network would be transmitted as a direct input to the AWS Univac 1108 computers in the Air Force Global Weather Central complex at Offutt AFB.

In developing the system, its designers drew from the huge reservoir of knowledge accrued over the years by solar astronomers, environmental physicists, and astrophysicists in both the East and West. Once the system is in operation, it will more than repay its debt to science. The system will be capable of processing masses of data on a real-time basis from a worldwide network of radio and optical observatories, all carefully calibrated and providing uniform reports. Military purposes aside, the system will give us a powerful tool for exploring our environment, providing researchers with a unique facility for studying not only solar-terrestrial relationships but many of the basic processes of the sun itself.

The simple prediction that a solar event will or will not occur is only the first task of the system. Later tasks will be to predict not only that a disruptive solar event will occur but its magnitude as well. There are large events and small events, with a continuum of sizes between. While the system concerns itself with all emissions contained in a solar burst, most of the attention of system planners to date has been focused on high-energy proton emissions.

The AFCRL-AWS Working Group is presently looking into the complex task of predicting not just the occurrence of a proton shower but its magnitude as well. The first problem the group encountered was the unsystematic reporting of the magnitude of solar proton events. Descriptions in terms of "major," "intense," "moderate," and "minor" are not very useful as parameters for a computer program. Discussion of this problem and its effect on other parts of the system led two AFCRL scientists in the group to develop a classification scheme, which they presented to the Inter-Union Commission on Solar Terrestrial Physics in London on 26 January 1969. The method classifies proton events in five categories of intensity. The scheme was favorably accepted by members of the commission and is likely to receive international standardized use by solar astrophysicists.

Solar protons are measured directly only by sensors in satellites above the earth's atmosphere. The first direct measurement was made by AFCRL in 1962 when photographic emulsion plates aboard Discoverer 17 were completely saturated by a dense flux of high-energy protons. The variety of sensors used since then has presented system designers with the problem they had faced from the beginning—lack of a common data reference base. With AFCRL's proton classification scheme, however, a fresh start can be made. The fresh start could consist of specially designed satellites for continuous monitoring of proton events. But specially designed satellites may not be necessary because ones adequate to do the task are already in orbit. These are the Vela satellites designed to monitor nuclear detonations in space.

The satellites will have several roles, some well defined, some uncertain. One possible

operational role is that of acquiring data for predicting the intensity, duration, and extent of a terrestrial effect once disruptive solar emissions have reached the vicinity of the atmosphere. But system planners are at present interested in satellites as research instrumentation for discerning system parameters. For example, after the satellite has detected a critical solar emission and its magnitude, we can look back to the combination of solar features that existed prior to the emission in an effort to uncover prediction criteria. Assuming such prediction criteria are perfected, then it may be possible to predict the magnitude of an event days or weeks in advance.

Satellites can also provide bench-mark data for ground instruments. Because intensity and duration of the terrestrial effect are correlated with the magnitude of the shower, we should be able to determine the magnitude of the proton shower indirectly by ground-based monitors. Riometers for measuring the opacity of the ionosphere constitute one such class of instruments. Another is the neutron counter. The solar proton classification method, noted earlier, takes into account these two instruments, showing that a proton event of a given magnitude can be correlated with the magnitude of a measurable effect detected by these ground instruments.

The Space Environmental Support System, then, is one that traces through an exceedingly intricate web of causal relationships among a diversity of environmental phenomena. The system has the curious feature of being a major system development in which the technology is available before there is a full scientific understanding of the problem. The technology is solved in the sense that most instruments presently thought to be needed for observations and for processing data are either in place or can be readily assembled. Reversal of the normal sequence, in which science precedes technology, dictated the unconventional Working Group systems-management approach. A centralized systems project office, with the function of setting specifications and schedules, would not have worked simply because you cannot direct a scientist to make a key discovery in order to meet schedules and maintain specifications. Even if solar and environmental physicists make no future discoveries, the Space Environmental Support System will fill an important need based solely on present knowledge.

Outside of a small group at AFCRL and AWS directly involved in the system, no scientific team was assembled for specific research on the system. The Working Group had to and will continue to rely on scientific discoveries as they become available. Because the problems are scientific and not technical, it is not altogether unlikely that some essential relationship that will greatly increase the capability of this Air Force system could result from a paper published by a Russian solar astronomer working, for example, at the Crimean Astrophysical Observatory. More likely, however, scientists at AFCRL will make many of the discoveries that will advance the capabilities of the system.

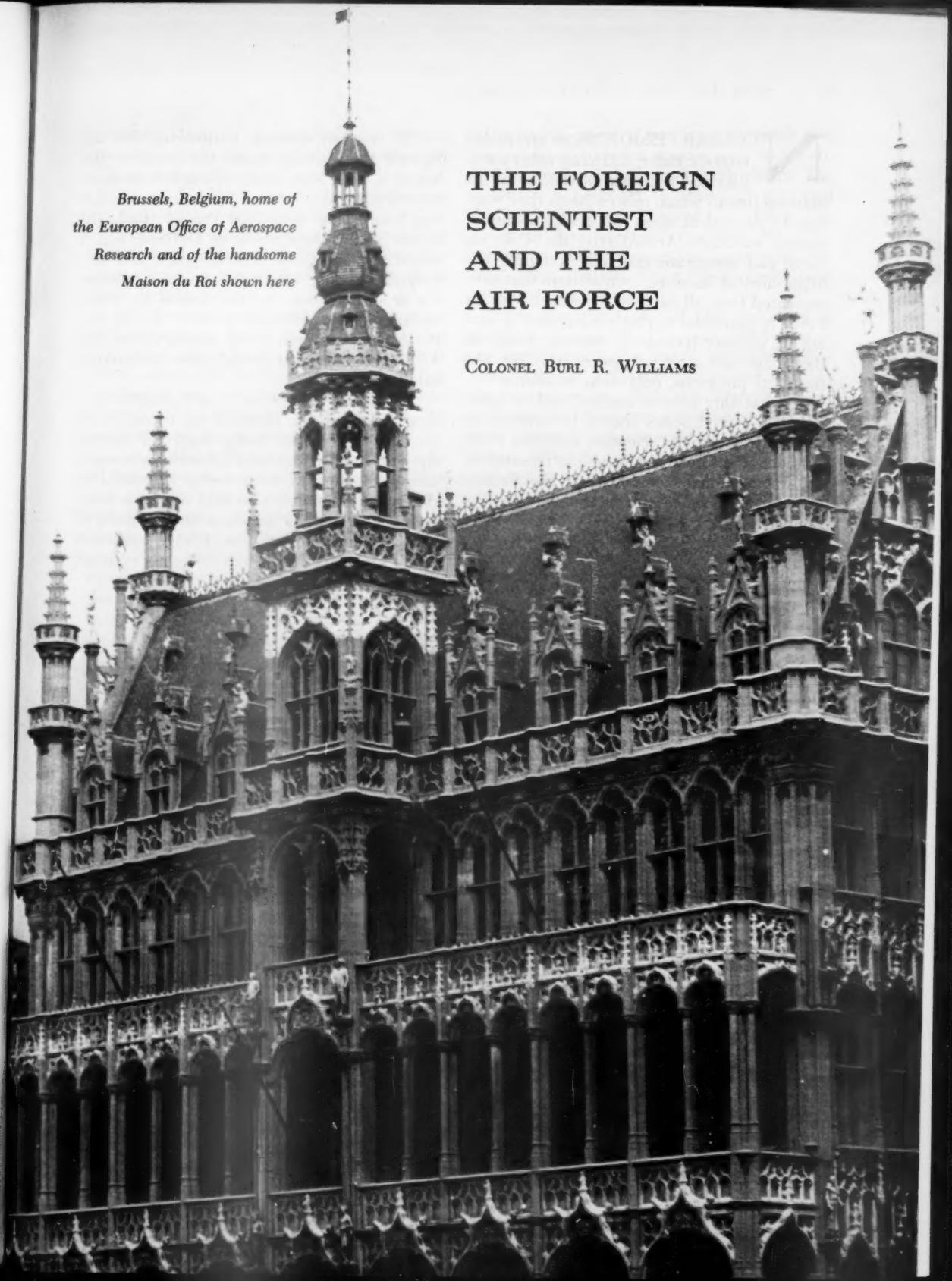
AFCRL is the Air Force center for environmental research—research that includes the dynamics and chemistry of the upper atmosphere, solar and radio astronomy, studies of the earth's magnetic field, the aurora, cosmic rays, charged particles in space and in the earth's upper atmosphere, astrophysics, and so on. The 250 or so scientists at AFCRL engaged in research that is in one way or another related to the space forecasting problem have available to them facilities that no other research laboratory in the world can match. In addition to its large solar and radio observatories, AFCRL each year instruments scores of rockets and satellites for upper-air and space research, and its five flying laboratories give AFCRL scientists the mobility to make measurements all over the world.

A further advantage, one that tends to be overlooked, is that because of their special skills and talents AFCRL scientists are called upon whenever the government or the DOD mounts large experimental programs involving the upper atmosphere. From the experiments there is often an abundant fallout of experimental data that are directly applicable to the Space Environmental Support System. Two examples of experiments that may seem far afield will show the kinship that exists:

• During the last series of nuclear tests in the atmosphere in 1962, about 150 AFCRL scientists with a vast array of rockets, aircraft, and ground instrumentation were on hand to participate. The data on atmospheric and near-space effects of nuclear detonations collected by AFCRL scientists have been essential to weapons and strategic planning by the DOD. After a lapse of seven years, in the summer of 1969, AFCRL, the Army Ballistic Research Laboratory, and the Defense Atomic Support Agency undertook a huge program in the arctic involving the launch of some 39 rockets, two of AFCRL's instrumented KC-135 flying laboratories, a satellite, and a range of ground instrumentation. These vehicles and instrumentation had been assembled to measure all phenomena associated with a polar cap absorption (PCA) event. PCA events—radio blackout in the arctic—are caused by high-energy solar protons. The same kinds of scientific specialties and the same general types of instrumentation were involved in both the 1962 and 1969 experiments because the environmental effects of high-energy charged particles from a nuclear explosion are much the same as those from energetic solar protons. A PCA event provides a kind of simulated nuclear explosion in the atmosphere. The studies conducted in the summer of 1969 were in no way a part of the Space Environmental Support System, yet the results are directly applicable, adding a further refinement to the computer program for correlating and analyzing environmental information.

• Some 30 to 40 AFCRL scientists participated in the Ft. Churchill, Canada, PCA experiments. Were they motivated in this work primarily by the desire to understand more of the environment we live in, or were they motivated primarily by the desire to improve Air Force operational capabilities? The question is meaningless, of course, and could not be answered even by those directly engaged in the experiments; their motivations are inextricably mixed. I would hope that the scientific component assays high in the mixture, however, because it is only through good science that such Air Force programs as the Space Environmental Support System will evolve to fulfill all the prediction tasks envisioned for it.

Air Force Cambridge Research Laboratories



Brussels, Belgium, home of the European Office of Aerospace Research and of the handsome Maison du Roi shown here

THE FOREIGN SCIENTIST AND THE AIR FORCE

COLONEL BURL R. WILLIAMS

NUCLEAR FISSION, the jet and rocket engines, radar, and many other scientific and technical advances have a common thread which relates them: they were heavily dependent upon the achievements of foreign scientists. Accordingly, the U.S. Air Force and Americans generally recognize the importance of scientific contributions that have emanated from all parts of the world. The Air Force in particular is vitally interested in having its military technology broadly based on the latest and most advanced scientific and technical programs, regardless of source.

It has long been recognized and accepted that the United States should be involved in some manner in the scientific activities being conducted in foreign countries. As reported by the International Committee of the Federal Council for Science and Technology in September 1964:

> This involvement is a fundamental necessity, because our scientific community cannot remain aloof from the world of science without paying a penalty in terms of wasted and ineffective effort and without missing new advances useful to our society. . . . There exist outside our borders unique opportunities in the form of natural conditions, unusual materials, unusually qualified people and special facilities.

At present, the Air Force has about 300 contracts and grants with foreign scientists, sponsored by Air Force laboratories through the European Office of Aerospace Research in Brussels, Belgium. Because of cost-sharing arrangements, the preponderance of the foreign research cost is borne by the foreign countries through their universities, national research councils, and other support agencies. Results of sponsored research are fed directly into Air Force research programs and are widely disseminated throughout the U.S. scientific community through the Defense Documentation Center and the published literature.

Air Force-supported foreign scientists are world leaders in their fields, as evidenced by the international awards and recognition they have received from their scientist colleagues. These foreign scientists contribute to the Air Force in all the basic disciplines: the physical, environmental, engineering, and life sciences.

It is often difficult, particularly for the layman, to ascertain exactly the actual or potential applications of the research that these scientists are conducting. The reason for this was pointed out in a 1966 Project Hindsight report by the Department of Defense, which stated that "delays of 5 to 10 years, or more, frequently occur between a scientific discovery or an invention and the time of its actual utilization." Nevertheless, it can be stated that the foreign research being supported by the Air Force does hold considerable technological promise.

A sample of contractors and grantees and their programs will illustrate the capability of the foreign scientists being supported by the Air Force. This particular selection was made simply to illustrate work being supported in different disciplines. It would not have been possible to make a selection on the basis of merit alone, since all the foreign scientists being supported by the Air Force are eminent in their respective fields.

Professor Porter:
rapid chemical reactions

Professor George Porter has long been recognized as an outstanding scholar, educator, and research scientist. He graduated from Emmanuel College, Cambridge University, in 1949, having studied under Professor Ronald Norrish, with whom he shared the joint Nobel Prize for Chemistry in 1967. After graduation he remained at Cambridge as

Assistant Director of Research in Physical Chemistry. Later he accepted an appointment as Professor of Physical Chemistry at the University of Sheffield, where his outstanding abilities led to his appointment in 1963 as head of its Chemistry Department. Since 1966 he has been Director of the Davy Faraday Research Laboratory of the Royal Institute of Great Britain.

Of the many honors that have been accorded Professor Porter, it suffices to say that the Nobel award represented the ultimate in world recognition for his scientific work on "studies of extremely fast chemical reactions effected by disturbing the equilibrium by means of very short pulses of energy."

As Professor Porter has pointed out, one of the principal activities of scientists has been the extension of the very limited senses with which man is endowed, enabling him to observe phenomena beyond his normal experience. Thus microscopes and microbalances allow observation of things of smaller dimension or mass than can normally be seen or felt. In the dimension of time, man is naturally limited to a minimum perception time of about one-twentieth of a second, the response time of the eye. Since most fundamental chemical reactions occur in the range of milliseconds or less, it is important that observations be made in microtime. It is in the field of developing techniques to make these microtime observations and interpreting the resulting data that Professor Porter is such an eminent authority.

The technique that has allowed Professor Porter and others to investigate the rapid reactions which are so fundamental to nature was conceived by him about twenty years ago. This technique, called flash photolysis, has been much improved by Professor Porter over the years. The principle is relatively simple, as is true of so many great scientific innovations. The short-lived intermediate chemical species of interest are produced by a flash of visible or ultraviolet light. The flash must be of sufficient energy to produce the desired measurable overall change and of short duration (approximately 10^{-3} second) compared with the lifetime of the intermediates. This latter requirement is necessary in order that the resulting transient species may be identified by means of their absorption spectra. The spectrum is obtained by means of a second flash, operating after a short time delay. The spectrum thus recorded can then be interpreted to yield information concerning the mechanism and kinetics of the photochemical reactions that occurred.

The limiting factor of this technique is the flash duration. In December 1968 Professor Porter reported a much improved apparatus utilizing a Q-switched pulsed laser as the energy source. With this innovation it is now possible to observe and study species that have lifetimes in the nanosecond (10^{-9} second) range. Although subnanosecond flash photolysis has not yet been attained, picosecond (10^{-12} second) laser pulses have been used in other studies, and there is every reason to believe the picosecond laser pulse is capable of being adapted to the flash photolysis technique for rapid-reaction studies.

The contractual relationship between the U.S. Air Force and Professor Porter began in 1957, when he proposed to conduct research on low-level excitation and energy transfer of gases. This field of study was of interest to the Air Force Cambridge Research Laboratories because of their in-house research in the fields of photosynthesis, organic semiconductors, and photogalvanics. His association with AFCRL continued, with the exception of one year, until 1967, when the Aerospace Research Laboratories (ARL) at Wright-Patterson AFB asked him to investigate the primary processes in organic materials. This work is continuing today, partially supported by the Air Force Office of Scientific Research (OSR) in order to complement their long-time research interest in photochemistry.

The research conducted by Professor Porter is a vital part of the OSR program in chemical energetics, which is concerned with various aspects of energy storage and transfer on the molecular level. This information is of interest because it helps define the electronic structure of the system and thus explain its properties. Further, it describes the energy prelude to chemical events such as isomeriza-

tion or cleavage. These chemical processes involve various beneficial and deleterious interactions of light with matter, and studying these processes will lead to better understanding and thus to improved control of them. This control is related to such Air Force needs as solar energy conversion devices, photographic imaging, flash blindness protection, and radiation damage control in biological systems. Professor Porter's unique experience and unrivaled capacity have afforded and will continue to afford further advancement in this vital field of research.

Professor Chauvin:
supersonic compressors

Professor Jacques Chauvin, one of the world's foremost authorities on supersonic compressors, is head of the Turbomachinery Laboratory at the Von Kármán Institute for Fluid Dynamics, an aerodynamics research facility in Rhode St. Genese, Belgium. The Institute was founded in 1956 by the renowned Dr. Theodore von Kármán and is under the auspices of the North Atlantic Treaty Organization. Professor Chauvin is a Belgian representative to NATO's Propulsion and Energetics Panel, has been a Visiting Professor at the University of Liége and the University of Brussels, and has frequently acted as a scientific adviser to research institutions and companies in the United States, France, England, Germany, and Belgium.

Professor Chauvin's research for the Air Force is aimed at developing a supersonic compressor in which the flow exceeds the speed of sound relative to one or more blade rows. This is independent of the vehicle velocity, which may be anything from zero to supersonic, depending on the specific application. Air Force interest in supersonic compressors relates to their potential performance, which is much greater than that of more conventional, lower-speed compressors. Supersonic compressors offer two special advantages: high pressure ratio per stage and the ability to accept higher subsonic inlet mach numbers than present compressors. The higher pressure ratio means that fewer compressor stages are needed, and therefore a turbojet engine can be smaller, lighter, less complex, and less expensive. The ability to accept higher inlet mach numbers simplifies the problem of designing inlets for advanced aircraft.

It has been known for many years that the key to obtaining these advantages was to turn the compressor at a higher speed. Of course, this is an understatement, and if that were all of the problem, it would have been solved long ago. In fact, an apparently straightforward approach was tried when jet engines were in their infancy but was soon abandoned when tests indicated that compressor efficiency and off-design performance were poor. The central problem is that, as the compressor blades are spun to higher rotational speeds, they are soon moving at the speed of sound with respect to the air through which they are passing. Thus the flow problems here are entirely different from conventional subsonic flow problems. The resultant shock patterns create starting problems, choking of the flow, and severe losses in efficiency.

In the early 1950s Dr. Hans von Ohain and Mr. Elmer Johnson of the Aerospace Research Laboratories conceived a blade approach which promised to overcome the problems experienced with more conventional blades and which potentially would yield

high performance over a wide range of mach numbers. This concept was based on diffusion of the flow by a multiple shock system, similar to the diffusion experienced in flow through a straight duct. This flow pattern is extremely complex, but certain characteristics of the flow have been well determined by experiments. One of the most important characteristics is that this flow pattern will always occur under broad and easily determined conditions. Also, diffusion of the shock in a constant-area duct permits operation without the extensive boundary layer separation and associated losses that occur in the conventional blading shock pattern.

A primary feature of this new blading concept is that the cross-sectional area of the flow passage is nearly constant from passage entrance to passage exit. This is accomplished by a continuous increase of blade thickness from the sharp leading edge to the trailing edge, giving the trailing edge a blunt appearance; hence the concept is called the "blunt trailing-edge supersonic compressor."

After this idea was conceived, the Aerospace Research Laboratories let a contract to Professor Chauvin to investigate the problem further. He was selected to participate in the project because of his important achievements in turbomachinery research. Also, the Air Force Systems Command's Arnold Engineering Development Center was called upon to test certain of the ARL designs. Thus a closely working three-member team has been active in the Air Force's development of supersonic compressors.

Professor Chauvin has contributed very important innovations. He allowed the channel between the blades to diverge slightly to allow for boundary layer buildup, and he rounded the trailing edges to minimize dump diffusion losses. Also his tests have created a better understanding of the influence of back pressure and distance between rotor and stator. As a result, he has developed a laboratory compressor with an adiabatic efficiency of 87 percent and with very good off-design performance, which is extremely important.

Professor Chauvin has made it a point to brief U.S. industry frequently on his developments, and new research designs by American manufacturers often reflect his work. Professor Chauvin is now further developing his ideas for the Air Force, in an attempt to achieve high mass flow rates. He and his American partners are laying the groundwork for substantially improved power plants for military jet aircraft of the future.

Professor Prigogine: statistical mechanics

Professor Ylia Prigogine is on the Faculty of Science of the University of Brussels. He has been engaged for many years in research in nonequilibrium statistical mechanics. The Brussels group established under his leadership is an international center for nonequilibrium statistical mechanics. Because of the many significant contributions made by him and his group in this field of research, Professor Prigogine has been accorded much international recognition. Among other honors, he has received the Prix Francqui and Prix E. J. Solvay, awarded in Belgium, and the 1969 Arrhenius Medal from the Swedish Royal Academy of Sciences. Professor Prigogine has served as visiting professor at leading universities, such as Harvard, Columbia, California, and Manchester, and has lectured at such international symposia as the Nobel Symposium. He has been accorded membership in leading scientific societies and academies in Belgium, France, Sweden, and the United States. Professor Prigogine has written or coauthored four books that have been standard texts in nonequilibrium statistical mechanics. In addition, he is Director of the Institute of Statistical Mechanics and Thermodynamics, University of Texas.

The Prigogine group is performing very basic pioneering research in nonequilibrium statistical mechanics. The general subject of statistical mechanics involves the study of the particular laws which govern the behavior and properties of macroscopic bodies, i.e., bodies made up of a large number of separate particles (atoms and molecules).

In principle, all the equations of motion of a mechanical system could be written down

and then integrated to obtain the motion of the mechanical system. There would be as many equations as there are degrees of freedom. Imagine the number of degrees of freedom in a single system such as a snow flake, which is composed of billions of atoms. The problem with solving such a system of equations is that the initial conditions are unknown. Even if all the initial conditions were known, the largest computer would be severely taxed to solve even the simplest imaginable problem, considering the time and paper required to print out the answer.

Fortunately, there is another approach to the problem, in which a mathematical transformation permits representation of the motion of a mechanical system in phase space. This is not a space in the usual three-dimensional way of considering ordinary space. With this new way of thinking, every system has a phase space with twice as many coordinates as there are degrees of freedom. At first it may appear that the problem has been infinitely complicated, until it is noted that a single point in phase space completely describes the state of a crystal. That is, the single point in phase space uniquely specifies the position and velocity of every atom of the crystal. Hence a significant shorthand has been achieved.

However, the initial conditions are still not known, so only a subsystem of the whole closed system is considered at first. Then the subsystem is permitted to interact with the entire system. After a period of time the subsystem will pass sufficiently through all its possible states. Based on this fact, it becomes possible to construct probability functions and thus determine the probability that a system will have a particular behavior. In this way many interesting functions, such as transport coefficients, hydrodynamic functions, and thermodynamic functions, can be calculated.

The Prigogine group has attacked the broader and more difficult problem of nonequilibrium statistical mechanics by considering the interaction of the subsystems. During the course of this research the group has developed new mathematical and analysis techniques. The results of this work are applicable to a wide range of physical situations of interest to the Air Force in thermodynamics, aerodynamics, hydrodynamics, ionized media, combustion, electric field effect, astrophysics, turbulence, and high-energy physics.

In 1966 the National Academy of Sciences —National Research Council of Washington, D.C., recognized the significance of Professor Prigogine's contributions as follows:

> The more complex problems of the fundamentals of irreversible statistical mechanics have been attacked only recently by Prigogine and colleagues at the University of Brussels. The understanding of the nature of the approach of equilibrium developed by the Prigogine school represents a contribution to the statistical theory of matter comparable in importance, perhaps, to the work of Gibbs.

(Josiah Willard Gibbs [1839–1903], America's foremost physical chemist, established the basic theory for physical chemistry during his approximately 50-year career at Yale.)

Professor Vassy: ionospheric studies

Professor Etienne Vassy is the Director of the Atmospheric Physics Laboratory at the University of Paris, France. He has held such prominent positions as Vice-President of the French Association of Engineers and Doctors, President of the French Committee on Scientific Radioelectricity, President of the Geomagnetism and Aeronomy Section of the French Committee of Geodesy and Geophys-

ics, Chairman of the Joint Commission of the Upper Atmosphere for the International Union of Geodesy and Geophysics, and Chairman of the Commission on Research of the Ionosphere for the Advisory Group for Aeronautical Research. He has been elected an Honorary Life Member of the Academy of Sciences, an organization that has honored him with three "Laureate" awards.

Professor Vassy is a key member of the Joint Satellite Studies Group. This group was formed upon the initiative of the Ionospheric Physics Laboratory at the Air Force Cambridge Research Laboratories shortly after the first artificial earth satellites were launched in 1957. The original role of this group involved the collection and correlation of data for more accurate prediction of satellite trajectories. The members of the group included working parties at observatories in the United States, Japan, Germany, England, Italy, Norway, Sweden, and France, and its original work was significantly important to Project Spacetrack.

During the course of the work for Spacetrack the group found that records of transmissions from satellites showed irregularities caused by heterogeneities of the ionosphere. Signals from the satellites suffered amplitude fading and phase changes in traversing the ionosphere. In effect, the ionospheric irregularities reduced the reliability of transmitted information. These findings were noteworthy, since satellites for communication and navigation were envisioned by the Air Force and other agencies.

To understand more fully the role of the ionosphere and its effects on electromagnetic wave propagation from satellites, the Air Force Cambridge Research Laboratories encouraged the Joint Satellite Studies Group to make a collective effort to derive a synoptic picture of the ionosphere from simultaneous observations of satellite beacons recorded at widely separated locations. Since the disturbing effects of the ionosphere result from different causes and each cause is associated with definite latitude regions, the original group was expanded to include at least one observatory on each continent. With the support of NATO, working parties were formed at observatories in Spain, Denmark, Greenland, Turkey, and Greece. In Africa and the Middle East working parties were organized in Ghana, Kenya, and Israel. Coordination of all activities was vested in the Air Force Cambridge Research Laboratories, and they called upon such prominent scientists as Professor Vassy to guide the efforts of the group. The various working groups are now cooperating to determine the characteristics of the ionosphere from the auroral regions through the middle latitudes to the equatorial regions.

The work performed by Professor Vassy and other members of the Joint Satellite Studies Group is very similar. Teams maintain recording watches to collect data transmitted from selected active beacon satellites. Principal satellites are the S-66 series orbiting vehicles and the synchronous satellites, ATS-1 and ATS-3. In the past, recordings were also made of transmissions from such satellites as Communications Satellite Corporation's Early Bird and Canary Bird, the U.S. Navy's Transit 4A, and AFCRL's ORBIS vehicles. Records of data are forwarded to the Air Force Cambridge Research Laboratories for special analysis. Much of the data is also distributed to various member scientists for analysis of particular ionospheric parameters such as total electron content and scintillation. In processing and analyzing the data, the group generates useful statistics for prediction of ionospheric conditions and the probability of low signal level occurrences as a function of magnetic latitude. These statistics are most essential and necessary in the design of satellite-borne systems.

Professor Vassy has personally made many contributions to systems designers through his Joint Satellites Studies Group activities. He has made a considerable number of measurements of the total electron content of the ionosphere and has given these data particular meaning by evaluating quantitatively the maximum possible error in the calculation of total electron content. He has developed a program to correlate total electron content with the period of the year, the solar time, the latitude, the elevation of the satellite, the

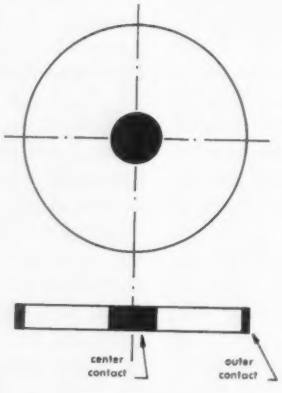
elevation of the sun, and the geomagnetic index. He has determined the trajectories of radio waves through the ionosphere, with and without the presence of the geomagnetic field, and has compared trajectory lengths with satellite-to-receiver distances. He has also performed these same studies using a specially developed "ray tracing" computer program. Another vital contribution by Professor Vassy resulted from his study of refraction using measurements of Doppler differential effects. The Doppler differential chain developed by him increased the precision of measurements by a factor of ten over previously used methods.

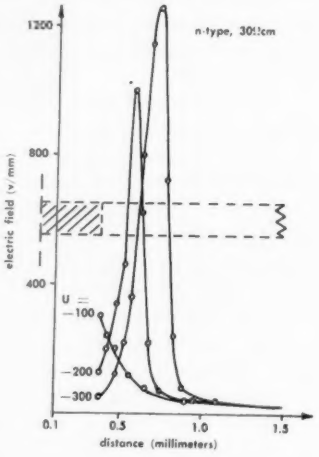
The Air Force uses the results of the work by Professor Vassy and other members of the Joint Satellite Studies Group for the design and evaluation of communication systems. DOD, FAA, COMSAT, and other agencies use scintillation data for evaluation of aircraft to synchronous satellite-to-ground communication and data links. NATO has an interest in ionospheric phenomena affecting a satellite communication system in development under NATO auspices. Observations of auroral data have been provided to the Lincoln Laboratory of Massachusetts Institute of Technology for evaluation of modulation systems for military communications. The U.S. Environmental Science Services Administration is utilizing scintillation information to evaluate a system for broadcasting frequency and time signals from a synchronous satellite.

The work of Professor Vassy and the Joint Satellite Studies Group continues. As new requirements are generated, new aspects of ionospheric phenomena will require research and evaluation.

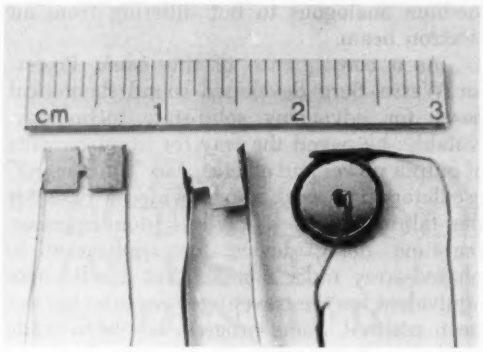
Professor Wessel-Berg: bulk semiconductor technology

Professor Tore Wessel-Berg is the head of the Division of Physical Electronics at the Norwegian Institute of Technology, Trondheim, Norway. Before assuming this position he was Head of the Microwave Electronics





The radial disc geometry and typical experimental observations of field distributions for several voltages.



Of considerable interest to Professor Wessel-Berg's investigations are coupled mode instabilities in finite samples of semiconductor crystals with nonuniform geometry. The two asymmetrical samples (left and center) were studied extensively in early work. More recent work has been on the radial disc-shaped device (right). Ohmic contacts with this germanium sample have been made at center and outer surface.

Group at the Norwegian Defense Research Establishment and a Research Associate in Microwave Electronics at Stanford University. While at Stanford he also served as a consultant to a number of U.S. industrial companies in the field of high-power extended interaction klystrons, transverse wave electron tubes, and relativistic electron beam theory.

During his early microwave tube research work at Stanford University, Professor Wessel-Berg formed a close working relationship with electronics research groups at the Air Force's Rome Air Development Center, New York. In support of an Air Force program, he served as an invaluable consultant in the development of a one-megawatt continuous-wave (CW) X-band extended interaction klystron for application in high-power radar systems. In conjunction with this work, he published a unified and comprehensive study of advanced theories concerning the wave phenomena of electron beams. Entitled "The Electron Beam as an Electromagnetic Medium," it is recognized as an invaluable treatise which will serve as a basis for research in years to come. By his early contributions Professor Wessel-Berg established himself as a leading authority in wave interactions and electron beam technology.

Following discovery of the "Gunn effect," which manifests itself as a microwave generation mechanism in the bulk of gallium arsenide samples when these samples are subjected to a critical d-c electric field, Professor Wessel-Berg reoriented his research to investigation of traveling-wave interactions in solid medium, particularly bulk semiconductors. These semiconductors possess high-power handling capability and other unique properties which depend on a solid volume of semiconducting material rather than a film or coating. In reorienting his research interests, Professor Wessel-Berg was motivated by the intriguing possibilities of utilizing drifting carriers in semiconducting materials as the primary energy source for traveling-wave amplification of microwave signals. He thus envisioned the development of miniaturized solid-state microwave devices and circuits for applications in advanced electronic systems, particularly phased-array radar systems.

Shortly after beginning this new work, Professor Wessel-Berg investigated the possibilities for active or cumulative mode interactions resulting in growing or amplifying waves in bulk semiconductors. While performing this work, he advanced unique approaches to the description of wave propagation and electronic phenomena in these semiconductors. In developing these approaches, he applied the coupled mode theory, a subject on which he is an expert, to the solution of transport processes in semiconductors by considering the semiconductor as an electromagnetic

medium analogous to but differing from an electron beam.

As a consequence of this work, Professor Wessel-Berg developed sound theoretical bases for advancing solid-state technology. Notably, he paved the way for improvements of output power and efficiency in "Gunn effect" oscillators. His work also provided a basis for the fabrication of a low-loss, nondispersive, real-time delay device for application in phased-array radar systems. Yet a solid-state equivalent for the traveling-wave tube has not been realized. Some progress has been made in electroacoustic devices; however, Professor Wessel-Berg is convinced that the most promising aspect for traveling-wave amplification is in direct-carrier electromagnetic interaction without the intermediary of acoustic waves. To realize fully the potentials of this type of interaction, he recognized that it was essential to understand the fundamental differences between the charge-modulation processes in electron beams and the collision-dominated carriers in solids. This is the work toward which Professor Wessel-Berg has now directed his attention.

In recent work sponsored by the Rome Air Development Center, Professor Wessel-Berg showed that carrier waves cannot be modulated at frequencies in the microwave region in the bulk of semiconductors with uniform properties; namely, materials like germanium, with constant values of mobility, drift velocity, and doping density. Doping density (that is, the amount of impurities diffused into the substrate materials, like germanium, to control the electrical properties) is an important factor governing bulk semiconductor characteristics. Professor Wessel-Berg found that carrier waves and electromagnetic waves do not couple in the bulk of uniformly doped samples, so amplification or cumulative gain cannot be achieved. As a consequence of this finding, Professor Wessel-Berg proposed a traveling-wave scheme involving periodic semiconductor configurations.

In preliminary investigations these configurations, which permit variations of doping density, show promise as a means to achieve a coupling of drifting carriers to electromagnetic waves, so that cumulative gain can be obtained. Professor Wessel-Berg plans to continue this work and the search for solid-state traveling-wave components.

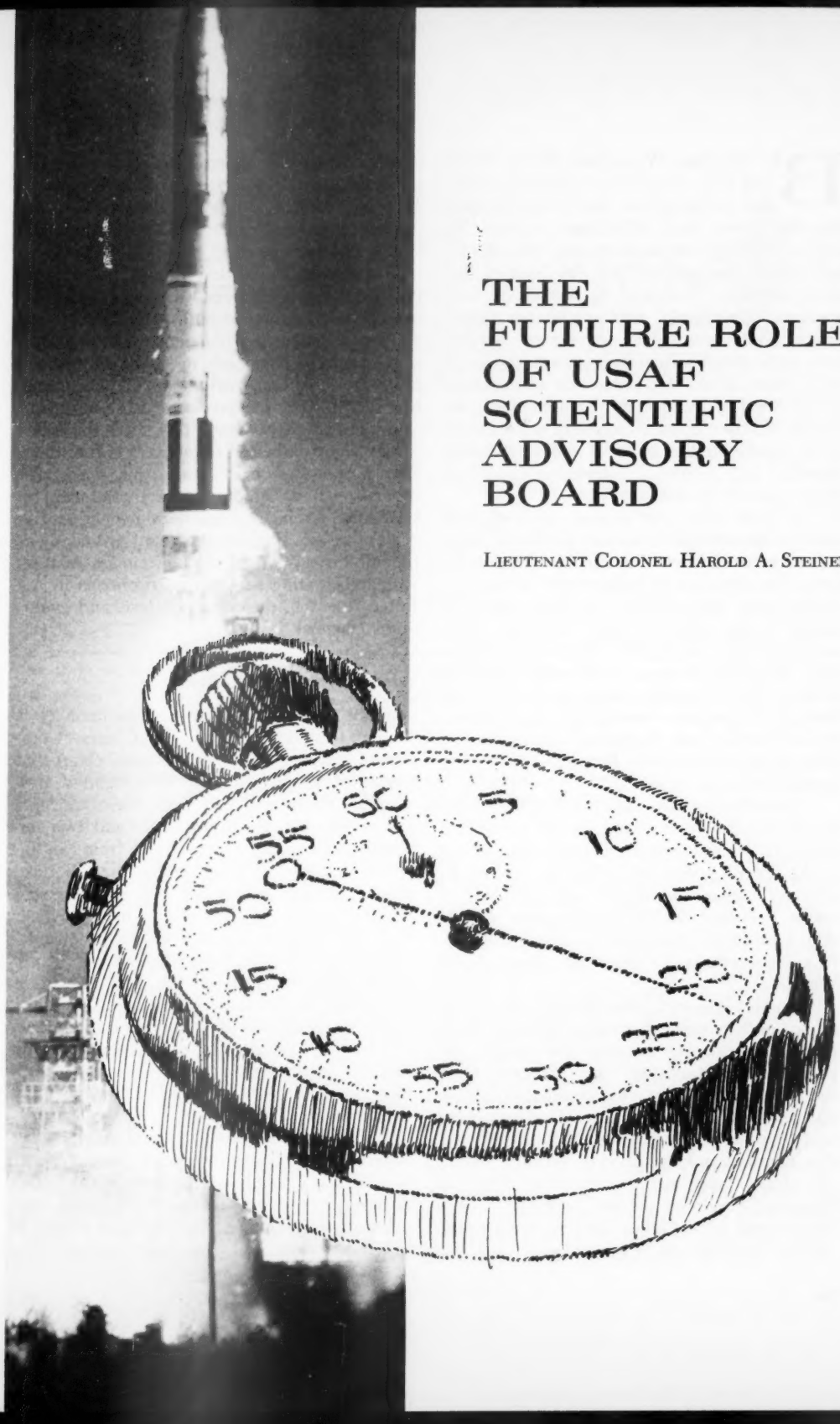
The work which Professor Wessel-Berg is performing is an integral part of an Air Force program dealing with the application of solid-state phenomena to perform conventional microwave device functions. Program emphasis is directed toward the use of solid-state devices to generate, amplify, and electronically control the delay of microwave energy for radar applications. The contributions of Professor Wessel-Berg will lead to the development of miniaturized, highly reliable devices to replace conventional microwave components, not only in radar systems but also in Air Force communications, countermeasures, and other electronic systems.

THE AIR FORCE is the beneficiary of high-quality research being conducted for it by gifted foreign scientists possessing unique capabilities. It is important that the relationship of the Air Force with the foreign scientist be maintained, since there will continue to be great scientific progress outside, as well as inside, the United States. Our technological and scientific self-interest, plus our desire to establish closer ties with our foreign friends, demands that we remain actively involved with these productive people wherever they may be.

European Office of Aerospace Research

THE FUTURE ROLE OF USAF SCIENTIFIC ADVISORY BOARD

LIEUTENANT COLONEL HAROLD A. STEINER



BY THE time World War II was drawing to a close it had become apparent to farsighted Air Force[1] leaders that the science and technology required to develop and operate modern weapons of war had become so complex that the "seat of the pants" intuition that had helped carry U.S. planners through the war would no longer suffice. The outlook was for future systems even more complex than those being employed at the time. If the Air Force was to maintain a high level of technological competence, research and development decisions would have to be made on the basis of more complete scientific and technical knowledge than had been available in the past.

At that time not enough sophisticated talent to provide the necessary technical judgment was available within the Air Force itself, hence the decision to supplement its R&D resource with advice from the best scientific minds in the industrial and academic communities. Accordingly, in 1944 the AAF Scientific Advisory Group, predecessor of the present USAF Scientific Advisory Board, was formed. Its purpose was to provide a forum for assembling and evaluating information on long-range R&D in the Air Force and to provide special studies as required.[2]

From the start the USAF Scientific Advisory Board justified the vision of its farsighted founders. It served as an effective instrument of Air Force policy and planning and in its advisory role contributed significantly to major decisions in the development of the deterrent forces that have enabled the U.S. to meet every major challenge since World War II.

Today, however, the Board's *modus operandi* and to some degree its influence have changed, primarily because the scope and pattern of military R&D have changed. Over the years the Air Force has developed competent in-house talent that can effectively cope with current R&D problems. More significant, the decision-making process through which the Board once made worthwhile contributions to the nation's defense posture has been pre-empted by a higher level in the Department of Defense. At best, the Board only gets involved at Air Force level. Consequently, it is mutually incumbent upon the Board and the Air Force to evaluate its functions in terms of the real world and determine how the Board's talents can best be used.

Any such evaluation should begin with the realization that, because of this increased competence within the ranks, advisory groups in the military departments will probably never again be as acutely needed as they were immediately after World War II. Accepting this fact need not depreciate their usefulness, however. In many ways the need for independent and unbiased opinions is still critical. The problem is to identify this need and determine how the Board's activities can be channeled to be of maximum benefit to the Air Force. Initially, one can begin by looking at the military, political, and economic matters that are creating the current problems in Air Force research, development, test, and evaluation (RDT&E).

current problems in RDT&E

The Soviets maintain that technology is the key to military power. The Chinese Communist leaders, on the other hand, claim that masses of people, properly indoctrinated, provide the decisive force.[3] Both these concepts are manifest in Southeast Asia, and two important lessons must be learned from our involvement there. The first is that the U.S. *must* plan continuously against the ever present possibility that the enemy can bring to bear both countless hordes and technological innovations against us in various parts of the world at multiple points of contact. And, as we are witnessing in Vietnam, these are not mutually exclusive. A supposedly poorly educated, backward people are proving quite adept at using modern technology against us even though they do not have the capability of developing it themselves.

The second lesson to be learned is that deterrence at lower levels of conflict (e.g., limited war) may be even more complex than deterrence of general war.[4] We pride ourselves on our Yankee ingenuity and ability to get things done, yet as a nation we are frus-

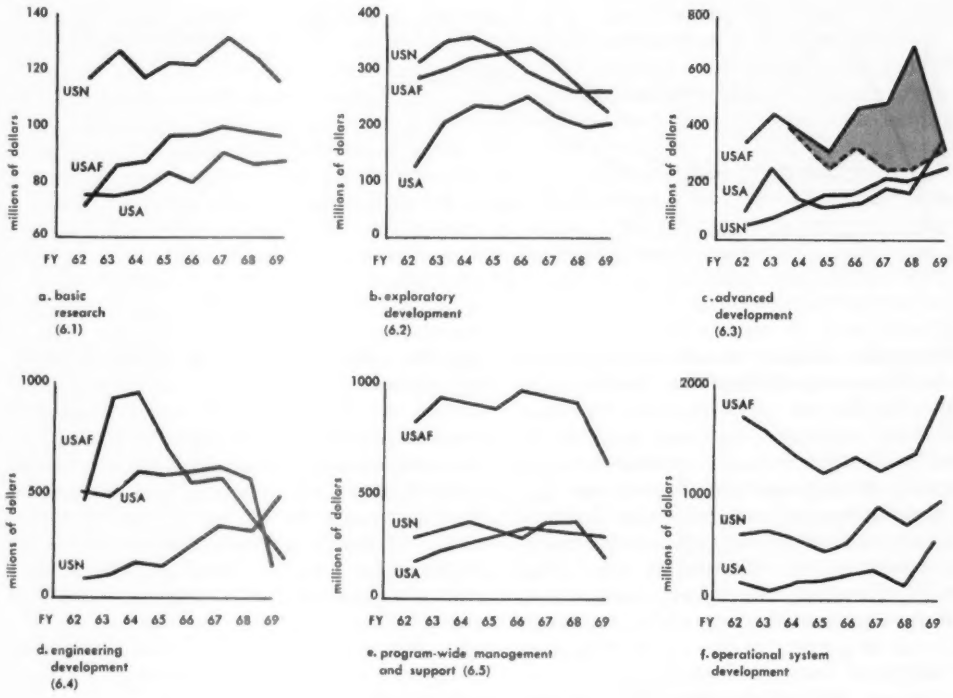


Figure 1. Critical problems of RDT&E (data extracted from Appropriations, Budget Estimates, etc. for the 87th Cong., 2d Sess., through the 90th Cong., 2d Sess.)

trated and baffled by our inability to handle a situation like Vietnam conclusively.

It is obvious, then, that the United States cannot continue to follow the time-honored American tradition of waiting until the last minute to attack these types of problems. Vietnam has proven the fallacy and pitfalls that accompany this tradition. Neither can the U.S. continue to redress the balance with weapons and tactics that are, at best, only marginally superior. To do so will result in casualties intolerable to the American people. Attempts to counter Communism must continue, but they also must be decisive and based on the advantage technology can give.

Technology provides the potential for many military systems, and each has its proponents. What is not clear at this point is which systems should be developed in the post-Vietnam decades, on what time scale, and at what cost in order for the U.S. to maintain effective forces that can compensate for the population imbalance.

Are the present RDT&E programs adequate to prepare the Air Force to help redress the balance in a decisive and timely manner? When one looks at the present R&D environment, the question answers itself. Despite arguments to the contrary, danger signals are flying, and the output of military RDT&E, both now and in the future, can best be described as amorphous. The complexities of the problem are shown in Figure 1, which compares recent trends in Obligation Authority for the six phases of military RDT&E. The impact of our involvement in Southeast Asia since 1964 is obvious. For the purposes of this discussion, the critical problems of RDT&E, the ones which

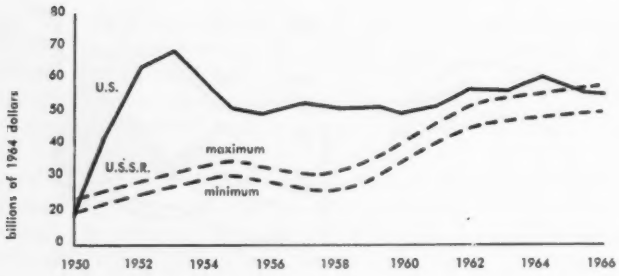


Figure 2. Comparison of U.S. and U.S.S.R. military/space expenditures in terms of 1964 dollars

the Scientific Advisory Board can best help the Air Force to address, are illustrated in Figures 1*a*, *b*, and *c*, which show how R&D funds have, in general, remained static or decreased since 1964. Figure 1*c*, which illustrates advanced development trends, warrants further explanation. It is obvious that the total appropriation for Air Force advanced development has increased substantially since fiscal year 65. However, the appropriations indicated by the shaded area, which represents well over 50 percent of the FY 68 obligation, are obligated for *one* high-priority system. The dashed line shows that the available funds for the other Air Force advanced development programs have been drastically reduced since FY 63. In FY 69 the advanced development (6.3) funds for all three military departments were increased slightly. However, the increase will only compensate for inflation, and Air Force advanced development remains in a depressed state.

Because of budgetary cuts, many Air Force elements involved in the phases of RDT&E illustrated in these figures have a difficult time carrying out the basic precepts of their mission, which is to explore and build the technology that the Air Force will need in the future. To paraphrase Dr. John S. Foster, Jr., Director of Defense Research and Engineering, "We are saving a few million dollars but our national security is losing a valuable asset."[5] Deficiencies in these phases of military RDT&E have little effect on present policies and decisions, but will have a profound effect on the policies and decisions necessary in the 1970s and '80s. It is conceivable, therefore, that the roles and missions of the military departments in those decades will have to be carried out with the forces and systems that are being developed and procured today, despite the obvious fact that "left over" weapons and "off the shelf" technologies have proven inadequate and ineffective as a means of decisively winning a war or discouraging an aggressor from starting one. This fact becomes especially alarming when one considers that the development time for new systems is approximately 10 years and that because of budgetary restrictions and priorities few new systems are even being started.

This deficiency in RDT&E can also be critical from a strategic point of view. At least with respect to nuclear war, the risks of not remaining equal with or ahead of a possible antagonist in terms of technology have never before been so great. Nor has the need for RDT&E funds to maintain parity in military technology been greater. Only the naïve can doubt there is a military R&D race between the Soviets and the U.S. Success will not be a one-shot proposition but will require a massive and uninterrupted R&D effort.[6]

As General James Ferguson has pointed out: "We have learned in recent years some harsh but perhaps enlightening lessons on the cost and consequences of allowing reaction or inaction to displace action as the moving force in the timing of national ventures."[7] If additional evidence is needed to confirm the seriousness of the situation, Figure 2 should help dispell any remaining doubt. The comparison of U.S. and Soviet military and space expenditures for the years 1950–65 shows the Soviets

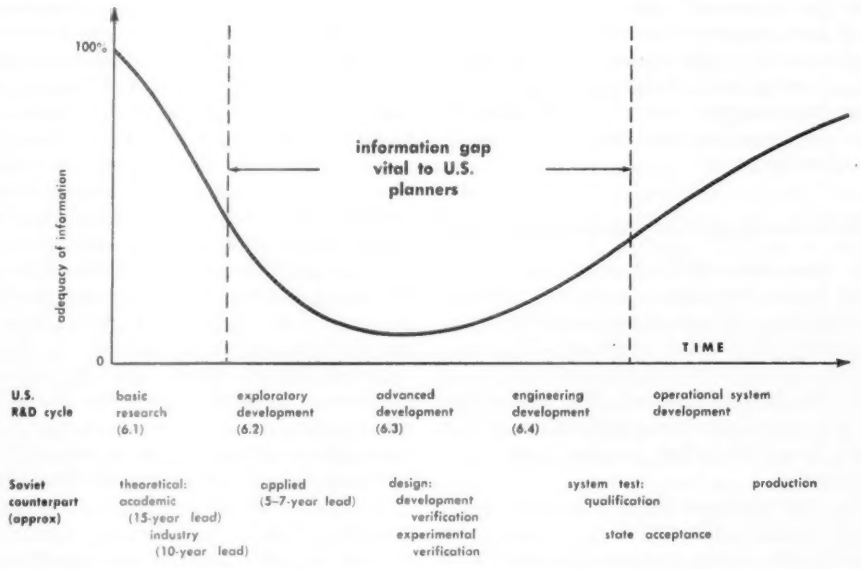
at first behind and then tending to catch up. By 1966 or 1967 the Soviets probably achieved parity or even surpassed the U.S. in non-Vietnam-associated expenditures.[8]

Although such comparisons are never completely valid, the U.S. expenditure for military R&D appears to be lower than that of the U.S.S.R. in terms of percentage of gross national product (GNP) and percentage of total defense spending, but higher in absolute terms.[9] What is of paramount importance is the fact that, although the Soviets' GNP is only about one-half that of the U.S., *their expenditures on priority items that further technological and military capability are comparable to our own.*

Until recently U.S. military technology has grown at a steady rate because funding, although perhaps not as much as the military would like, has been adequate to build a sound technological base. However, because of Southeast Asia today and proposed high-priority nonmilitary commitments for tomorrow, it is doubtful the nation can continue to fund military R&D at the maximum rate. Therefore, optimum use must be made of the R&D funds available. This is an area where the Scientific Advisory Board can help the Air Force.

The obvious countermeasure to Soviet technology is for the U.S. to maintain insurance with healthy programs in research and exploratory and advanced development in a variety of areas that can quickly lead to operational systems to counter any Soviet threat. However, this cannot be a random process; we must anticipate where the big payoffs will be. This is especially important because the Soviets have a built-in advantage in the technological race: whereas U.S. intentions are well publicized and immediately available to Soviet decision-makers, the U.S. must make R&D decisions on the basis of inadequate information. Figure 3 illustrates schematically that our information on the Soviets' RDT&E decreases significantly when they begin to

Figure 3. Information gap concerning Soviet weapon system development



exploit basic and applied research for weapon system development.

The critical problem, and the one on which the Scientific Advisory Board could perhaps best help the Air Force, is to project our technology and military potential into the obscure area where our knowledge of Soviet exploratory and advanced development approaches zero. Uncertainty as to what the Soviets are doing in these areas tends to delay U.S. counter decisions until actual Soviet disclosure or tests reveal a new or improved technological capability. If this philosophy of action/reaction is carried to its logical conclusion, undoubtedly at some future time the Soviets could unleash a disastrous strategic technological surprise just as there have already been irritating tactical technological surprises.

To summarize this argument, research and development are never done in a vacuum. Neither can they lie fallow and then spring to full instant productivity. The research necessary to provide a sound technological base is a function of time. Once lost, it is lost forever and can never be recovered. Trained cadres of scientists and engineers and continual cross-pollination of their ideas must be maintained in both industrial and governmental laboratories if R&D programs are to be productive and ready to serve the national interest when needed. Getting behind in technology is the one absolute thing the Air Force, the Department of Defense, and the nation must make every effort to avoid.

post-Vietnam economic and political environment

In some R&D circles there is an almost pathetic belief that once we are out of Vietnam conditions will return to some semblance of normalcy. This belief is illusory—there are many indications that this will not happen, at least to the degree expected. The underlying force that generates these indications is the pressing need for urban renewal and social improvement.

The post-Vietnam era will continue to be a period of disequilibrium as more and more special-interest groups become concerned with the formulation of policy.[10] There will continue to be sharp conflicts between domestic, foreign, and military policies. Although the external political environment will have a significant effect on strategic decisions, many major military decisions will be made primarily in response to domestic political pressures.

As soon as Vietnam ceases to be a major problem, the plans and objectives of domestic policy makers will undoubtedly prevail. A strong undercurrent for social improvement permeates society today, not only in the U.S. but in the entire world. The belief is strong that available resources must be used for the betterment of mankind.

The nation has no choice but to address itself to this goal. National self-interest requires that the U.S. undertake massive development programs to improve the lot of its citizens. Next to maintaining a strong strategic deterrent, social improvement is probably our most critical problem and the greatest challenge our nation faces.

None of these programs will be cheap. Billions of dollars will be required for urban renewal and social improvement. In one sense such expenditures represent a long-term investment, but for the short term it is clear that the money must come from somewhere. Since tax increases are unpopular, the major source of money available after Vietnam may very well be the military budget. Congress will be under increasing pressure to decrease the military expenditures to pay for domestic programs. Therefore, we can predict with some assurance that resources for military R&D will continue to be scarce after Vietnam, perhaps almost as scarce as they are now.

Competition for R&D dollars will be strong. No longer will the Air Force enjoy the favored position of the 1950s and early 1960s, when the strategic retaliation forces were being developed and procured. The Department of Defense in general and the Air Force in particular must seek an accommodation that is compatible with an environment and economy oriented toward civilian needs. The Air Force must plan now on how it can achieve and maintain a viable and dynamic military force in such a political and domestic environment.

Does the USAF Scientific Advisory Board have a future role?

As indicated, the critical problems in current research and development and in the projected military, political, and economic environment dictate that the Air Force use every means at its disposal to maximize its contribution to the national military posture. Within the context of this discussion the question is, Does the Scientific Advisory Board have a role in helping the Air Force achieve this capability? The answer is an unqualified yes.

The next problem is to determine how that Board can be made more effective. The Board was originally charged to assemble and evaluate information on long-range R&D. Because we are now challenged for first place, this precept is more valid than in the past, especially in view of the implications of the current technology race between the Soviets and the U.S. The deficiencies that can result from a technology gap need to be identified by some knowledgeable, unbiased agency, such as the Board, and their potential impact on both Air Force and national interests documented.

A step in this direction could be made if the Board were to embark upon a multidisciplinary effort aimed towards projecting technology into the future and identifying potential problem areas where R&D directed towards solutions must not be allowed to falter. Such an effort should be open-ended and on a continuous basis.

Some of the present Board members should be retained and would form the nucleus of the technology effort. But what would be of paramount importance and vital to the ultimate success of the effort would be the judicious augmentation of the Board with bright young scientists and engineers who are advancing to the forefront of their specialty fields. Such an arrangement would also provide these young scientists and engineers with a greater opportunity to become familiar with the inner workings of the Air Force. This by-product would be expected to pay significant dividends in the years to come.

With this personnel mix, the Scientific Advisory Board would have the benefit of maturity and judgment combined with up-to-date working knowledge of technology. It is envisioned that the technology groups would keep constantly apprised of current developments in their fields and would meet at individual working sessions several times a year to evaluate and document current technology and changes in the state of the art as it applies to the Air Force.

As now, there should also be at least two general Board meetings per year where all the Board members could meet with Air Force personnel to strengthen their identity with the Air Force and be briefed on current plans and programs. In turn, the Air Force could expect yearly dividends in meaningful recommendations as to which new opportunities should be pursued in order to help the U.S. develop and maintain a credible deterrent posture.

Any rearrangement or reorientation of effort would not depreciate the Board's ability to perform the second part of its mission, to review Air Force programs and make special studies as required. Reviews and special studies could continue to be made on an ad hoc basis, drawing members from the entire Board as required.

In addition to scientific and technological forecasts, the Air Force needs a review mechanism that is not submerged in day-to-day problems. The so-called "think tanks" and not-for-profit organizations, which are sometimes charged with this responsibility, are not always able to provide an objective review because of strong personal interests. In many cases the individuals who are giving the advice helped formulate the plans and programs on which they are advising. In such a situation built-in biases are hard to overcome.

Concomitantly, with this review mechanism one of the greatest advantages the Board provides to the Air Force is intangible and cannot be measured. It is derived from the Board's serving as a catalyst when the principals involved in Air Force problems get together during the Board's reviews and deliberations. Ideas for improvement and courses of action become apparent during the informal discussions. Often these discussions lead to changes that are implemented by the Air Force

without formal guidance from the Board. In this catalytic capacity the Board provides an immeasurable asset to the Air Force.

In order to insure that the Board uses its limited resources to work on the proper reviews or special studies, there is one additional management process that can significantly improve the Board's effectiveness. Regularly the Board hierarchy, consisting of a minimum number of people to insure candidness, should meet with Air Force leaders to discuss current and projected Air Force problems. In this way the Board could obtain continuous high-level guidance that would enable it to respond in a more timely manner to problems of major concern. In other words, the Board must be recognized as a valuable resource, respected as such and used as such.

SOME of the problems that military R&D must contend with today have been shown and an attempt made to project the military, political, and economic environment that will affect military R&D in the post-Vietnam era. Some of the critical aspects of letting our technology slip have been pointed out. Finally, it has been shown that the Scientific Advisory Board does have a role in helping the Air Force to circumvent post-Vietnam problems. In so doing two major areas have been identified for Board action—projecting technology and special studies.

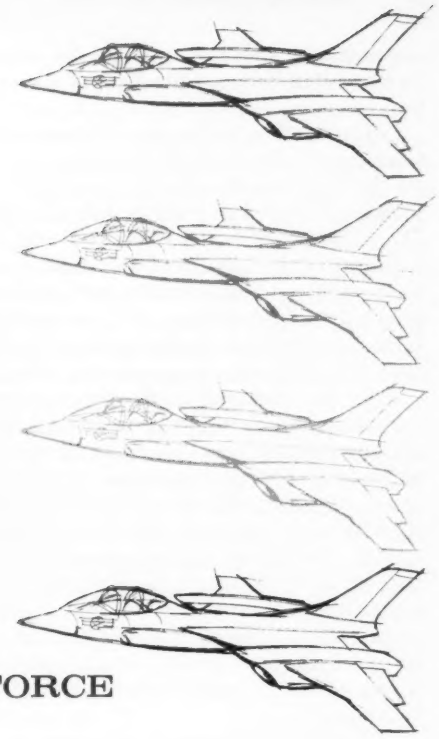
To be successful in these endeavors, the Board must remain viable by having the right people. It is equally imperative that the Board attract attention from the highest Air Force levels, where there is visibility through the entire Air Force R&D and operational structure. With these two attributes—vital people and proper guidance—the Scientific Advisory Board can continue to play its needed role and achieve its full potential.

USAF Scientific Advisory Board

Notes

1. Although of later origin, the term "Air Force" is used throughout.
2. *The USAF Scientific Advisory Board—Its First Twenty Years, 1944–1964,* USAF Historical Division Liaison Office, GPO, Washington, D.C., 1965.
3. William F. Scott, "The Contrast in Chinese and Soviet Military Doctrines," *Air University Review,* XIX, 2 (January-February 1968), 57–63.
4. The Center for Strategic Studies, Georgetown University, *The Soviet Military Technological Challenge,* Special Report No. 6, Washington, D.C., September 1967, p. 6.
5. Statement before Congress in February 1968 concerning the defense R&D posture.
6. *An Analysis of the US-Soviet Strategic Interaction Process (U),* Strategic Studies Center, Stanford Research Institute, California, final report, 2 vols., 1967 (SECRET).
7. James Ferguson, "Foundations for the Future," speech to Los Angeles Chapter, American Ordnance Association, 15 February 1968.
8. Stanford Research Institute, *An Analysis . . .,* p. 10.
9. The Center for Strategic Studies, *The Soviet . . . Challenge,* p. 40.
10. Disequilibrium—a period of controversy and change; there is more public discussion and partisan debate; disharmonies and fissions develop.—Samuel P. Huntington, *The Common Defense* (New York: Columbia University Press, 1961).

Air Force Review



EQUIPPING TOMORROW'S FORCE

MAJOR GENERAL WILLIAM G. MOORE, JR.

ONE DAY not too many years from now the first F-15 air superiority fighter will be delivered to the Tactical Air Command. This aircraft will represent a new capability designed to counter the increasingly sophisticated manned fighters of the Communist nations. It will also represent the end product of a study advocacy, development, and acquisition sequence that to many persons is a sort of unfathomable "black magic." Probably the least understood phase of this activity is the "requirements process," which begins with the recognition of a force deficiency and extends to the approval of new programs, hardware modifications, or other actions leading to a solution of the problem. This initial step more than any other part of the sequence determines the capabilities the Air Force will attain ten to twenty years in the future. It is important for all of us to understand how the requirements process provides new force capabilities such as the F-15 or the B-1. From my experience as the Director of Operational Requirements and Development Plans in the Air Staff, I may be able to explain the process and dispel any notions that there is "black magic" involved in this or in any other aspect of the development and acquisition sequence.

To begin with, the procedures for initiating development of new force capabilities are spelled out in Air Force Regulation 57-1, "Policies, Responsibilities and Procedures for Obtaining New and Improved Operational Capabilities." The *Air University Review* of January-February 1968 contained an article by Colonel Geoffrey Cheadle, "What Is an Operational Requirement?" which described very well the intention of this regulation. There has not been, however, a published in-depth review of the working aspects of the

requirements process as might be obtained from reading between the lines of the regulation.

The guiding light that illuminates consideration of the most effective force structure for the future is the document entitled, "USAF Planning Concepts," which is revised annually. "The Plan" sets forth either explicitly or by implication the objectives to be achieved in the next fifteen years but is in itself too broadly oriented to be sufficient for management of force developments. Rather it forms the broad base on which requirements are structured.

To be meaningful, the force objectives must have the certification of senior Air Force decision authorities. There must be some agreed understanding as to the relative importance of these objectives if they are to be directly useful to force planning. In practice, specific development programs seldom originate as a direct result of "The Plan" or the objectives contained therein. But those programs which are initiated are related in the force structure reviews to the long-term goals of the Air Force as set forth in "USAF Planning Concepts."

Specific development programs for new Air Force system capabilities usually originate as reactions to deficiencies or lack of adequate capabilities for the job to be done, discovered during force employment or from mission evaluation studies. These studies take two forms: examination of force capabilities or application of new technologies. A problem uncovered during tests or force employment normally has a more direct path to resolution than a study recommendation for two reasons: (1) the need is more apparent, and (2) AFR 57-1 provides direct access to reactive elements of the Air Staff. Studies, on the other hand, seldom generate a real sense of urgency by themselves. However, the less stylized review arrangements often result in early presentations to high Air Force management levels, which can result in action if sponsorship develops during the briefings. In these cases the quality of both the briefing and the study may exert considerable influence on the action taken in response to the study conclusions. One of the difficult management tasks in the requirement process is neutralizing persuasive program sponsors so that all candidate projects for Air Force dollars can be viewed from an equally objective base.

Mission evaluation studies are intended to examine force performance capabilities in terms of one or more of the Air Force objectives. These examinations lead to a knowledge of the deficiencies, if any, in the present or projected force structure for accomplishing the mission reviewed. In this sense, the term "mission studies" is often too narrowly interpreted as a theoretical paper exercise. Anyone who looks for a better way to do his job is in effect doing a mission evaluation, and his thinking may lead to new equipment or procedures. For major force changes, of course, the mission studies are considerably more detailed than is possible from a single person's viewpoint. The result of any mission evaluation is a determination as to whether there is a need to change tactics or equipment. If an equipment change is indicated, it is fed into the requirements process as a Required Operational Capability (ROC) under AFR 57-1 procedures. Basically, the ROC should describe the new standard of equipping performance that is required or, in other words, what is needed. Since this document is fundamental to structuring the future forces, I will comment in some detail on it.

A determination that must be made for any new development program is its relative importance to the overall capabilities of the Air Force. In this regard it is extremely helpful when the originator of a ROC describes how new equipment will contribute to the mission task of his command and the urgency of acquiring the improved capability. In practice, of course, no ROC is staffed without close coordination between the submitting command and the responding organization. However, this coordination is facilitated by early understanding of the criticality of the solution to the planned operations of the submitter.

A second point regarding ROC's is that the deficiency must be expressed in such a way that there is no confusion on the part of the responder. Two situations occur most often: (1) The ROC states the problem in terms of a

solution; it describes "how" to solve the problem and not "what" the problem is, thus reducing the alternatives available in correcting the problem. (2) The problem is stated so broadly that a large and expensive study program is required to reduce the alternatives to manageable numbers.

One additional comment regarding AFR 57-1 procedures is appropriate. A number of variations to the basic ROC process have been created to facilitate support of special critical mission areas. Quick Reaction Capability (QRC) for handling electronic warfare and reconnaissance requirements is one of the most firmly established. Another is the Southeast Asia Operational Requirements (SEAOR) procedure for handling requirements submitted by Seventh Air Force in SEA. When a deficiency is determined by the operating unit (Seventh Air Force), it is reported electrically to Hq Pacific Air Forces (PACAF), Air Force Systems Command, Air Force Logistics Command, Tactical Air Command, and the Air Staff. The responding command, AFLC or AFSC, commences a review immediately and provides the Air Staff with a Best Preliminary Estimate (BPE). A BPE is the result of a quick analysis and evaluation of what has to be done, and it states the preliminary solution to the problem, including an estimate of cost and schedules. PACAF, in order to reject the stated SEA requirement, must comment negatively, or the efforts toward a funded solution will continue. Such a system does provide quick solutions. For example, early in the SEA conflict, the electrical power in Vietnam had a tendency to fluctuate, resulting in erratic operation of electronic equipment. A SEAOR was submitted, and within several weeks voltage regulators had been shipped to alleviate the trouble. While this problem, fortunately, was resolved with off-the-shelf commercial hardware, requirements are not often satisfied so readily. Usually some development work is necessary to provide the best solution for the operational forces.

In Hq USAF we have become aware that there may be a need to process ROC's on the basis not only of urgency for a solution but also the cost of the program that will be initiated. Presently, all ROC's are processed under one set of guidelines as defined by AFR 57-1. Yet probably 70 percent of the requirements are for relatively low-cost items that are important but not absolutely essential to the future operation of the forces. Many of these, while not urgent, are so relatively inexpensive that they do not deserve a comprehensive staff review. Very likely there are actions that can be taken to streamline the staffing of such low-cost projects. The result would be to facilitate the overall management of requirements by concentrating on ROC's that call for urgent, expensive projects. We are considering this possibility. Before making any hasty judgments, we must recognize that the total of low-cost projects, if accorded direct access to funds, would impact on the initiation of higher-value programs. In other words, we cannot afford to approve all low-cost projects without regard to the total of Air Force requirements for new equipments. The question is, what is the proper balance of management attention to these smaller projects versus the major weapon system requirements?

In the foregoing, projects and programs are referred to incorrectly. In the normal chronology there would not be a project or program yet identified at this stage, only the expressed requirement. A project or program becomes part of the process only after the concerned elements of the Air Staff and the involved commands agree that action should be taken in response to the ROC or other expression of an operational need. This is not a failure to recognize the very fine work carried on by our research and development agencies on a continuing basis. Rather, I am merely pointing out that until we have identified the approach that best satisfies the requirement stated in a ROC we are not in a position to seek funds for the resultant project or program.

For this reason some further study is always required to determine the best solution to a requirement. To accomplish this, Hq USAF either conducts or directs a mission/concept study to define *how* the problem is to be solved. Again the term "study" should

not be interpreted narrowly because often the solution is so apparent that a cost and feasibility examination is all that is required. For major new systems the study may continue for a year or more, with considerable contractual support and the associated substantial expenditure of funds. These mission/concept studies develop alternative solutions and define the equipment performances necessary to meet the standards identified in the ROC. The possible solutions are interrelated by mission, costs, technology, and effectiveness. It is important that a full range of alternative concepts be explored. The conceptual studies should describe in hardware terms the systems and subsystems that are potential candidates to close the gap between existing capabilities and the required operating standard. We always seek participation by the originator of a requirement in these studies.

The conceptual studies probably indicate a need for major advances in technology or even a technological breakthrough prior to an acceptable solution. Therefore, the gap between the desired mission performance and the existing capability cannot be closed immediately. In this circumstance, the mission for which the requirement was submitted must be re-examined to determine if interim fixes providing lesser capabilities are acceptable.

Up to this point in the requirements process, matters are very straightforward: determine what the Air Force needs and start the staff actions to provide it. Once there is agreement to proceed toward a solution, however, the system gets considerably more complex, since a number of optional approaches may have to be considered.

Often a requirement can be satisfied by a simple equipment modification. Funds are available each year to accommodate these changes, and the major challenge, outside the normal paper work, is to place the available funds against the most urgent projects and turn the task over to AFLC. An example of the modification procedure is the work the Logistics Command is doing on the C-119 gunships. Electronic warfare requirements can be met by application of technology through the Quick Reaction Capability arrangements. The QRC program has been used to develop equipment to counter the surface-to-air missiles (SAM's) in SEA. These two methods are unique in that funds are available in a limited amount to be spent at the discretion of the Air Force. In the normal situation we must "advocate" our proposed solution program through the Air Staff and the Office of the Secretary of Defense (OSD) to obtain necessary funds. This, of course, is an area where many differences of opinion are likely to be overpublicized.

The normal advocacy process usually starts with one or two concept studies prior to concept formulation. The distinction between concept formulation and conceptual studies is very simple: In the latter we are concerned with relating any number of possible solutions to the requirement through a general evaluation of the mission and the technology. In concept formulation we are comparing selected alternative solutions in a rather formal way (realistic costs, schedules, etc.), looking toward making a recommendation on the preferred solution to OSD for approval.

As activity shifts from the original problem to some preferred solution, the task of advocating or selling the program increases in scope and tempo. Each problem has its own best solution, and each solution has its own sales problems. In every case some individual or individuals must exhaust all possible means of ensuring that the program is given fair consideration in relation to all others when decisions on the allocation of funds are made. This is, in fact, a real exercise in perseverance and staff agility and not at all an exercise in "black magic."

One of the complicating factors with which we as advocates must contend is the ever present funding problem, which has several facets. First is the relative cost of new systems. The sophistication and complexity of our proposed systems have resulted in extremely high development/acquisition costs. These costs are reflected in the Air Force budget as added investments. As a result, although the Air Force budget has increased

over the past several years, a substantial portion of that increase is needed to pay for purchases approved when they cost fewer dollars. For instance, the C-5A investment costs have grown, taking a larger slice of available funds. Secondly, the SEA conflict has increased the operating cost for our forces. These factors have operated to limit the research, development, test, and evaluation (RDT&E) budget to a relatively constant figure of $3.5 billion for the past several years.

However, a constant dollar budget for RDT&E programs does not provide a true picture of what is actually happening in the technical development area. Not only are the SEA operations increasing Air Force operating costs; monies used for normal technical development are diverted to provide quick solutions for SEA operational requirements. Further, the inflationary trend in our economy has resulted in a decrease in the value of available dollars. Thus less research and development can be purchased with the same number of dollars. For example, when the programmed RDT&E funds for FY 63—FY 68 are deflated for the increased cost of conducting research and development activities, the Air Force RDT&E budget can be shown to be effectively reduced by 30 percent.

The final facet of the funding problem is the fact that a gap already exists between the Air Force RDT&E estimate of funding to satisfy stated requirements and the approved Five Year Defense Program. Considering that future prospects of increased funding for the Department of Defense are slight and recognizing the existent RDT&E gap, one can perceive the difficulty that an advocate faces in justifying new systems. Despite the severe competition for funds, several major Air Force programs have been successfully advocated recently, e.g., B-1, SRAM, AWACS, and F-15. These systems will require large expenditures of RDT&E funds over the next few years. Our success in advocating these systems will increase the difficulty in obtaining future systems.

One more point will clarify the requirements process, and that is in regard to advocacy. What is meant, in practice, by this term? It means that we respond to all the regulations, staff offices, individuals, procedures, etc., that contribute to obtaining a positive decision to fund the recommended Air Force program. This includes the necessity of a complete discussion by all elements of the Air Force as to the value of alternative solutions, with the concomitant detailed investigation and resolution of the many factors bearing on the final version of a proposed program. It means much frustration, long hours of overtime for rewriting staff papers, periods of frenetic activity in preparing briefings, rechecking system design details, etc. Whom is this activity directed toward? The answer depends on the size and type of the development program. For major weapon systems, of course, the approval authority rests with the Secretary of Defense. This means that all the interested staff agencies and offices below this level down to the major commands must be fully acquainted with and in agreement with the proposed program. No precise staffing procedure is followed, but eventually the Air Staff Board, Air Staff Council, Chief of Staff, Assistant Secretary of the Air Force R&D, and the Secretary of the Air Force must in turn approve the program before it is forwarded to the Secretary of Defense. When the proposal reaches the OSD staff, actions such as responding to Development Concept Papers, Draft Presidential Memorandums, and other key staff papers are normally required. These actions require meticulous, detailed attention due to their potential impact on the decision to proceed with or disapprove the advocated program.

At this juncture let me amplify an important point: once the Chief of Staff has approved a particular program, it serves no beneficial purpose for any element of the Air Force to continue voicing reservation or disagreement. On occasion Air Force interests have been jeopardized by overzealous persons publicly proclaiming their own version of a new system while we are pushing approval for the preferred Air Force design. It is very important that all of us express our concern and honest convictions regarding new ideas, but not after a best solution has been determined

and accepted by the proper authorities.

Appointment of a single manager for a proposed program focuses the numerous essential activities on the overall objective of obtaining expeditious development decisions. The staff officer charged with this responsibility may be of any rank. Large programs that markedly affect the future of the Air Force may have a general officer appointed as the single overall manager, as for the F-15 and the B-1 programs. Any officer who has negotiated a program through the entire sequence, from receipt of the requirement to final approval for acquisition, deserves recognition by his fellow officers.

These steps and restraints are the framework within which we in the Directorate of Operational R&D Plans pursue the necessary programs to ensure the continued effectiveness of the operating forces. Our task is to create order and progress from a continuum of hundreds of new requirements each year. We have learned from experience that the dynamic nature of supporting sciences and the demands of an ever varying Air Force mission preclude the establishment of one "grand plan" to achieve the order and progress we seek.

AN ANALOGY to the Air Force requirements process can be seen in our day-to-day personal activities. All of us have at one time or another mentally chosen some goal in life that we wished to attain, the object of our ambition. Furthermore, that position evolved from our present status and the knowledge gained from past exposure to the environment around us. This dynamic view of our ambition in the world is analogous to "The Plan" in the Air Force. Our personal "Plan" contains objectives that we want to reach. In this sense a constant mission evaluation occurs so that our goals are evaluated with respect to our present capabilities, to determine "what" is needed to ensure their accomplishment. These "whats" or additional capabilities may range from the austere to the excessive, from material to spiritual. What are the possible solutions to the deficiencies we recognize in our lives? Where and how can we obtain the means to remedy them? Seeking answers to these questions leads immediately to a comparison of alternative methods for reaching our goals. The alternatives pose different funding problems and very likely quite different completion times. Nonetheless, the alternatives are compared, and the best choice is made within the limits of our mental and financial capabilities. Having made a choice, we must then advocate the solution, to our parents, possibly to our wives, or to the banker whose low-interest loan is crucial to proceeding. (I hope this simplistic but true analogy has not again raised the specter of "black magic.")

IN THE Air Force, the problem of ordering goals and related requirements is that any listing of goals is never absolute. Too many variables enter into determination of the listing, and these variables—threat, technical feasibility, urgency, etc.—are interpreted from different viewpoints. The requirements process in the Air Force and DOD is built on innumerable judgments or decisions, which are in turn based on interpretations of the data base. Further, these judgments change with evolving circumstances. Achieving order requires a dynamic working format. This is where requirements planning becomes necessary.

We have under way at the present time a thorough study of requirements planning. Our approach might be called the worm's-eye view, since we are in effect working from the level of considerable detail toward the broad perspective of Air Force objectives. The purpose of this re-examination is to create a continuing planning capability that provides three outputs:

a. A direct correlation between known deficiencies and ongoing or new research and development programs.

b. A fuller appreciation of the basis available to support decisions on major system programs.

c. A more easily understood relationship between apparently disparate Air Force missions, such as counterair and assured destruction.

In working terms this means starting with mission areas selected for their understandable relation to the force structure but sufficiently narrow in scope to be manageable. Within these mission areas a determination is made as to the appropriate measures of effectiveness. The outstanding requirements are then viewed against these measures, to determine their relative contribution to Air Force capabilities in that mission area. As one would suspect, there is a considerable need for judgment in this exercise, since performance criteria that clearly distinguish comparative capabilities seldom exist. If they do exist, they are almost never universally accepted by all persons in the decision chain. However, knowing the agreed measures of force performance is in itself a partial solution to the requirements planning problem because such knowledge reveals the latitude remaining for judgments regarding the particular solution to the requirement. This information strengthens and focuses the arguments that are part of the approval process for the new project.

If the force capabilities can be weighed within selected mission areas, there is hope that relative evaluations can be made between mission areas. The idea is to expand our understanding of the role and contribution of each element of the force until the pieces integrate into the whole.

Whether we are successful depends on the diligence we apply to this formidable task; I have little patience with skeptics who say it is impossible. We are making decisions relative to equipping the forces every day. Additional understanding derived from detailed study certainly cannot detract from the decision process. Further, I do not agree with the view that all planning must start from broad objectives. If we can develop methods for viewing a limited portion of the Air Force mission, it will certainly contribute to better understanding of the "big picture." In some instances it may influence the expression of objectives. We must find ways to relative ordering of our programs if we are going to make best use of available dollars toward achieving the greatest overall force effectiveness.

One further thought concerning the whole requirement-development-acquisition sequence. The staff officer who has followed his program along the tortuous path from conceptual studies to engineering development is most knowledgeable in both the requirement and the preferred solution. He has perforce become committed to the program. Although there are prescribed procedures for transferring programs between Air Staff elements at various phase points in the development/acquisition sequence, the complete involvement that a staff officer has with his program cannot be transferred. His involvement and expertise only further the progression of the program towards development for the inventory. Having decided that the best interests of the Air Force are served by keeping staff officers with their program throughout the development cycle, two procedures can be adopted towards accomplishing this objective. The staff officer could be transferred to other elements of the Air Staff as the program progresses between elements. Alternatively, the program could remain in one office throughout its development cycle. Of the two alternatives, the latter should be considered the more acceptable, for this reason: Under the former, Air Force personnel requirements not related to the program can cause a change in assignment of staff officers. These assignments can occur at unpropitious times in a program development cycle, thus greatly reducing the expertise and commitment to the system or program. On the other hand, if the program had remained within one office of the Air Staff, the project officer's involvement would have been assimilated to some extent by other officers in that same office through the normal dialogue. In this way, loss of expertise and commitment caused by reassignment of the project officer would not be nearly as great.

Therefore, I propose that responsibility for systems/programs remain in one office from the initial Air Staff response to a requirement until resolution of that requirement. This would be in line with the concept of charging a staff officer with the responsibility as was done for the F-15. This would also be in line with a proposed reorganization of the DCS/R&D,

whereby the total study advocacy, development, and acquisition sequence for a given program will be under one Air Staff Director.

It is important to remember that through the entire sequence there is one continuous thread: *responsiveness to the needs of the operating forces.* While the task of responding to operational requirements principally involves research and development activities, the first principle in structuring programs is to accommodate the needs of the operating forces. An important condition is that no *final* decisions on new programs are made without major command inputs.

In short, the requirements process in the Air Staff is designed to construct a bridge between the operating forces and the technical community. In the requirements planning process we try to develop better understanding of the factors that bear on decisions for future force equipage. Requirements planning also provides a continuous evaluation of requirements and responses. If we do all these things well and pursue our program with a singularity of effort, the Air Force will continue to be the most effective fighting force in the world.

Hq United States Air Force

CHAPLAINS ON THE FLIGHT LINE

CHAPLAIN (COLONEL) ASHLEY D. JAMESON

FOR several years, the chaplain's flightline ministry has been stressed by commanders as that part of an Air Force chaplain's work that contributes most to the morale and spiritual well-being of USAF personnel. In re-

sponding to the plea to get from behind their desks and out to where the action is, chaplains have found the key to a much better overall ministry. Airmen are much more likely to attend chapel if they know the chaplain, and the flight-line ministry affords a chaplain the opportunity to get acquainted with the airmen. The counseling opportunities of the chaplain increase in proportion to the time and effort he devotes to visiting with the people where they work. Anyone seeking advice or assistance with a problem feels much more at ease in asking assistance from a friend. An expressed concern for people and desire to befriend them results from an effective flight-line ministry.

Originally flight-line ministry was spending part of each day visiting with the men on the line. It meant mixing with the men who fly the planes, fix the planes, schedule the planes, and so on. It meant getting to know the people, giving them a chance to know their chaplain, and expressing a concern for the men and their mission. Now it has come to mean much more than that. It means a counseling session in a borrowed office, in a corner of a hangar, or at the snack bar. It means religious services, on Sunday or a weekday, in the training room, the alert room, a maintenance area, even on a bus. It means a word of encouragement, reassurance, or comfort according to the need, when and where the need is recognized. In short, it means practically all of the chaplain's work being adjusted to the airman's working area, but often it is only the beginning of a job that is later completed in the chaplain's office. Yet it is a job that would not have begun if the chaplain had not first gone to the flight line.

The words "flight line" describe the ministry rather than designate its location. To the chaplain the flight-line ministry simply means visiting and serving the men where they work. "Flight line" was first used to identify this work primarily because the mission of any operational air base is accomplished on the line, where often men are working 24 hours a day. So, to the chaplain, the line is the heart of the base, and that is where he has to be. The needs of the men on the line are no different, however, from those of the airmen in the supply section, personnel section, or any other area of the base. The flight line has a definite priority in the chaplain's work plan, but in reality the line includes every airman on the base. His concern is for the airman, and it does not matter whether he has an F-4, a typewriter, a wrench, or a computer card in hand.

The chaplain on the flight line is not trying to take the chapel to people who do not come to chapel. His effort is not to force a sermon on someone who is not attending the chapel services. The purpose is not to recruit a congregation. He is doing what the Air Force is paying him to do, being a pastor to Air Force people. The spiritual well-being of the people is the chaplain's responsibility, and he cannot do much for them without spending a lot of time with them. He may go there to talk with a specific person, or persons, about a particular matter. Or he may just go visiting in the area, making himself available to the men, expressing a concern for them, trying to identify with them. The chaplain has learned from experience that his counseling opportunities are not so much dependent on how many problems the men have as upon how well he is known and trusted by the men. In order to know and be known by the men and to win their confidence, he is willing to engage in a lot of light chatter and drink many extra cups of coffee. The chatter and the coffee are not parts of his mission, but they are vital elements in accomplishing his mission. They open the door to many eyeball-to-eyeball preaching and counseling situations.

There are many different types of organizations on the flight lines of Southeast Asia. There are fighter squadrons, bomber squadrons, the Jolly Green Giants and other rescue groups, the psychological warfare units, the medevac organizations, Ranch Hand crews, forward air controllers, the cargo and passenger-hauling units, the reconnaissance pilots, maintenance people, refueling crews, armaments personnel, and others.

Each has its specific mission; it has its own lingo, its own spirit. As the flight-line chaplain moves from one organization to another, he is conscious of the differences in

The Air Force chaplain's flight-line ministry includes worship services on the line . . . in the mobile chapel . . . and an encouraging word wherever an encounter provides the opportunity.

them. Each one thinks it is the greatest and that it has the most important job to do. The chaplain enters into the spirit of each group as if he felt the same way. Each one offers its own challenge to him, and if he meets it effectively, he is accepted as a member of the team. There are many opportunities for the chaplain's services in all these groups—after he has won their friendship and confidence.

The chaplains who work with the casualty staging units in Vietnam are well aware of the uniqueness of their opportunities. Few ministers ever have the opportunity to mean so much to so many men as these chaplains do. They deal with men who are in pain, men who are frightened, worried, and confused. These wounded men have just engaged the enemy in battle. They have been air-evacuated, to be airlifted to a hospital in Cam Ranh Bay, Japan, the Philippines, Hawaii, or back home. Wherever there is a casualty staging unit, the chaplain meets the patients at the flight line, visits them in the casualty assistance area, and accompanies them to the flight line when they leave. Reassurance is given them regarding their flight and the continued care they will receive. The pastor comforts them, jokes with them, or prays with them according to their desires. They express their desires in various ways, and the trained ear gets the message. No chaplain has a more difficult task than this one, nor one more rewarding.

There are many very small flight lines in Vietnam, some with only a dirt or metal landing strip and a helicopter pad. These small forward locations are visited regularly by a chaplain from the nearest Air Force base. He

hitchhikes air transportation out to see the men and gets back to his base the same way. He may spend 4 hours or 34 hours at the site, but while he is there he conducts religious services, counsels those who seek his help, and visits the men. The chaplain always receives a warm welcome at these locations, and he considers his visits more than worth the time and trouble of getting out there and back.

Regular visits to the security guards who man the perimeter guard posts constitute a vital part of the flight-line chaplain's ministry. Most of these men work alone at their posts for long hours. Time passes slowly as they look out across the rice paddies, fields, villages, and forests. A few words by radio with the central control room now and then are the only communication they have with anyone. Of course, quiet and a lack of excitement are just what the guards want. But the days and nights are long in this work, and the chaplain's visit provides a welcome break. The pastor-in-uniform gets by to chat with these men at all hours of the day and night. He answers many questions and provides much counsel. He also does a lot of listening because lonely men like to talk.

A unique feature of the flight-line ministry is conducting religious services away from the chapel for men whose duties make it impossible for them to attend regularly scheduled services. Such services take on a variety of forms and places. The chaplains at Nakhon Phanom, Thailand, use a base bus equipped with an altar and appropriate literature. Originally begun during Lent of 1968, the chapel-bus has been so enthusiastically received as to require continuation of this ministry on a daily basis. Catholic and Protestant chaplains alternate the devotional services, which reach approximately a hundred men daily at six different stops.

Chaplains find an excitement in working with the tactical flying organizations on the flight line that is found nowhere else. Men here risk their lives day after day in their regular job performance. They go after the enemy, his supplies and his transportation. Each of them wants to fly as many missions as he can while in Vietnam, but he is also aware that every mission he flies could be his last. He cannot think about it much, but neither can he completely forget it. When the flight-line chaplain visits him just before a mission, there are usually no words spoken about prayer and faith. But there is a presence that conveys the thoughts very effectively—an expressed concern as the airman leaves and a joyous greeting when he returns. When a plane returns home with severe battle damage, among those men anxiously waiting and praying for its safe landing is the pilot's pastor, the chaplain. Sometimes, of course, a plane is so badly damaged the pilot cannot possibly bring it home and has to eject. Soon thereafter, two chaplains visit the commander and crewmen in two organizations. One is with members of the downed pilot's organization; the other is visiting with the crew and members of the rescue organization that will attempt to get the downed airman safely home. These are tense and anxious minutes—often hours—for the men in both these groups. The chaplain is with them. He is a member of the team and a reminder of the eternal concern of God, who is "not willing that any should perish."

Hq Pacific Air Forces

THE AEROSPACE RESEARCH PILOT SCHOOL

MAJOR DONALD J. KUTYNA

Using a 6000-pound-thrust rocket engine mounted above the regular jet exhaust, Air Force Flight Test Center's NF-104A can attain altitudes over 120,000 feet. Designed for use by students at the USAF Aerospace Research Pilot School, the aircraft has hydrogen peroxide jets in the nose and wings to provide maneuverability in the rare portions of the atmosphere. . . . Computers feed data to simulators that enable students to become familiar with an aircraft without actually flying it.

FLIGHT-TESTING of piloted aircraft has been going on since the day the Wright brothers made their first flight more than sixty-five years ago. Just as "the proof of the pudding is in the eating," so also must the proof of an airplane be in the flying, for engineering will always remain in part an empirical science, subject to verification by actual test. Thus, although the aeronautical sciences have undoubtedly enjoyed vast development during the past fifty years, this development has not eased but rather has intensified the need for flight-test work and, in turn, the need for trained flight-test and research-test pilots.

Flight-test training in the Air Force originated as an in-house activity in Test Operations at Wright Field, Ohio, about 1943. It progressed to formal school status, with its own staff and aircraft, and in 1951 moved to Edwards AFB, California. Events of the late 1950s disclosed the need for aerospace pilots trained for work in advanced aircraft and manned space research programs. Thus in 1961 the research pilot or "aerospace training" phase was added to the Test Pilot School's curriculum. Now called the Aerospace Research Pilot School (ARPS), it is constantly changing to keep its course of instruction, test methods, and training vehicles ahead of the anticipated needs of future test programs. People who were associated with the school just a few years ago would hardly recognize it today.

The purposes of any flight-testing of

piloted aircraft are to determine the actual characteristics of the machine (as contrasted to the computed or predicted characteristics); to provide developmental information; and to obtain research information. The particular mission of the Aerospace Research Pilot School is to train experimental test pilots to supervise and conduct flight tests of research, experimental, or production-type aerospace vehicles; and to train aerospace research pilots for flight test, engineering design, and/or management in advanced aerospace research programs.

To enlist the highly capable pilots desired to work eventually in the field of flight test, the school and the Air Force maintain an exceptionally rigid selection process. The minimum eligibility requirements are a bachelor's degree in engineering, physical science, or mathematics; application to be made before 32d birthday and the course entered before 33d birthday; applicant must be an active duty pilot in the grade of major or below, with a minimum of 500 hours as instructor or first pilot in jet or turboprop bomber or transport aircraft, in supersonic fighter or trainer aircraft, in helicopters, or in a combination of these.

While these minimum requirements must be met even for consideration, the large number of applicants allows final selection of pilots with much higher qualifications. In the last four graduating classes the average backgrounds of the 16 military members of each class included ten bachelor's degrees, five master's degrees, and one doctorate; average age of 31; and 2300 hours' flying for the average fighter/trainer pilot (11 pilots per class), 2800 hours' flying for the average bomber/transport pilot (5 pilots per class).

Diversity of experience is an important (and in today's Air Force a rare) attribute sought for in a prospective test pilot. The majority of the pilots selected for the school have extensive operational experience in three or more aircraft. In addition, experience and subsequent assignment considerations dictate the desirability of a combat tour in Southeast Asia prior to assignment to the school. Thus almost all recent entrants have completed Vietnam tours.

Phase I - Experimental Test Pilot Course

The eight-month Experimental Test Pilot Course is designed to train pilots in the latest methods of testing and evaluating aircraft and related aeronautical equipment. The academics, simulation, and flying training are designed to give the student the theoretical and practical background required to supervise and conduct flight tests of research, experimental, or production-type aircraft.

Phase I is divided into two major segments, Performance, and Stability and Control; they are approximately three and five months in duration, respectively.

Performance

The Performance subphase progressively develops the theory, flight-test techniques, and data-reduction methods associated with performance flight-testing.

Academics. The academics are conducted at the level of an undergraduate aeronautical engineering course, beginning with a brief review of calculus, physics, thermodynamics, and aerodynamics and progressing through the detailed theory of aircraft and engine performance and testing. The time allotted some of the subjects is extremely limited. The calculus review is just two hours long and in reality consists only of a final exam covering an entire year's course in calculus. Physics-thermodynamics is allotted only six hours to cover all applicable concepts. This brevity of the basic courses is dictated by the ever increasing complexity of flight-testing and the greater number of subjects to which a new test pilot must be exposed to prepare him to understand and perform his work. The factor which allows pushing the student through the basics at this rapid pace is the student's own high-level academic background—most of those accepted for the school had a B± or better average in related courses at the undergraduate or graduate level.

Flying. Performance flying closely parallels the academic curriculum and teaches the student the basic methods of performance data gathering. Airspeed and altimeter calibration,

Flights in the Schweizer 1-26 glider complement the pilot's experience in high-performance aircraft. . . . Cornell Aeronautical Laboratory's B-26 variable stability trainer provides a vast range of flight characteristics.

takeoff, climb, range, acceleration, turning, and descent performance are all investigated. To apply his knowledge to a practical exercise, each student flies a limited performance flight-test program in either the T-33, T-38, or B-57 and documents his results in a formal written report. Individual oral briefings on the progress of the test program give each student the experience to practice this form of reporting.

Stability and Control

The Stability and Control subphase is designed to prepare the student to test and evaluate the handling qualities of an aircraft.

Academics. Background theory courses in vector analysis, differential equations, operational math, and dynamics prepare him to derive the basic equations of motion of an aircraft. The equations are examined in detail, with in-depth studies of each of the important terms that influence aircraft stability. Considerable time is spent on the theory behind roll coupling, spin testing, and aeroelastic effects. The expanding role of electronic and hydraulic systems to aid or completely implement the stability and control of modern aircraft has dictated the inclusion of an extensive group of subjects related to linear and nonlinear control theory. More than fifty hours are allotted to this area of study. In all, the student spends 475 hours in the classroom during the Phase I Performance and Stability and Control portions of the curriculum.

Flying. The uniqueness of the ARPS curriculum lies in the coupling of the academic courses with the school's computers, simulators, and aircraft. As an example, to investigate the stability and dynamics of an aircraft, the student first derives the basic aircraft equations of motion and becomes familiar with the variables involved. These equations are placed on an analog computer, where the effects of varying basic frequencies and damping ratios can be observed visually. The school's static aerodynamic simulator is then used to allow the student to vary individual stability parameters to see how they change an aircraft's handling characteristics. Having acquired this basic familiarity with the parameters involved, the student takes to the air in Cornell Aeronautical Laboratory's B-26 variable stability trainer and

the two ARPS NF-106 variable stability aircraft. These flying laboratories allow an infinite variance of aircraft handling qualities and control systems, and they give the student first-hand in-flight experience in observing a vast range of flight characteristics that could never collectively be found in individual test or production vehicles. Finally, the student applies his newly gained experience to the planning, conducting, and reporting of a relatively complete stability and control investigation of the F-104 and either the T-33, T-38, or B-57. In addition to the basic stability measurements, the investigation includes an examination of the "engine out" characteristics of the B-57, the spin characteristics of the T-33, and the high supersonic mach number handling characteristics of the F-104.

V/STOL. The Stability and Control portion of ARPS training also includes an extensive introduction to the emerging field of vertical- and short-takeoff-and-landing technology. Both helicopter and V/STOL theory are coupled with helicopter flight in the H-13 and UH-1F, and V/STOL flight simulation in the Ryan XV-5 simulator. Future plans call for advanced helicopters and V/STOL training aircraft to be added to the curriculum.

other areas

Another area which receives heavy emphasis in Phase I is qualitative flight-testing. Each student is briefed on the optimal techniques to be employed and then performs a one-flight evaluation of one or more aircraft that he has never flown before. Aircraft available for evaluation span most of the Air Force inventory and even include some Navy and allied vehicles.

Finally, the pilots are given a two-hour introductory glider flight program to complement their high-performance experience with very high-lift flight. The introduction consists of eight flights in the Schweizer 2-32, 2-33, and 1-26 at a nearby gliding facility. In addition to gaining a basic familiarity with aircraft of this type, the student gathers limited data on glider handling and performance.

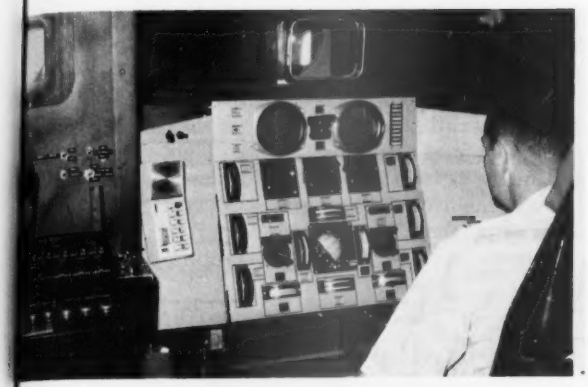
Probably the most tedious yet one of the most necessary parts of the curriculum is the written and oral report program. In addition, the individual analysis of the test data required to prepare a report is a powerful tool in helping the student develop a critical eye for the effects of flying accuracy on test results. A test pilot who cannot clearly and accurately report his findings would be wasting valuable time and effort in flying a test program. Thus opportunities are given the student to practice and develop his writing and speaking abilities.

AS THE END of the Phase I curriculum approaches, the student begins to gain considerable understanding of the language, theory, techniques, and problems of flight-testing. Thus at this time he is given increasing opportunities to learn through a series of lectures by various experts in fields associated with flight test. In addition, each class is given field trips to various test activities and aircraft manufacturers, and one class each year travels on a two-week visit to study the flight-test centers and schools of our allies in Europe.

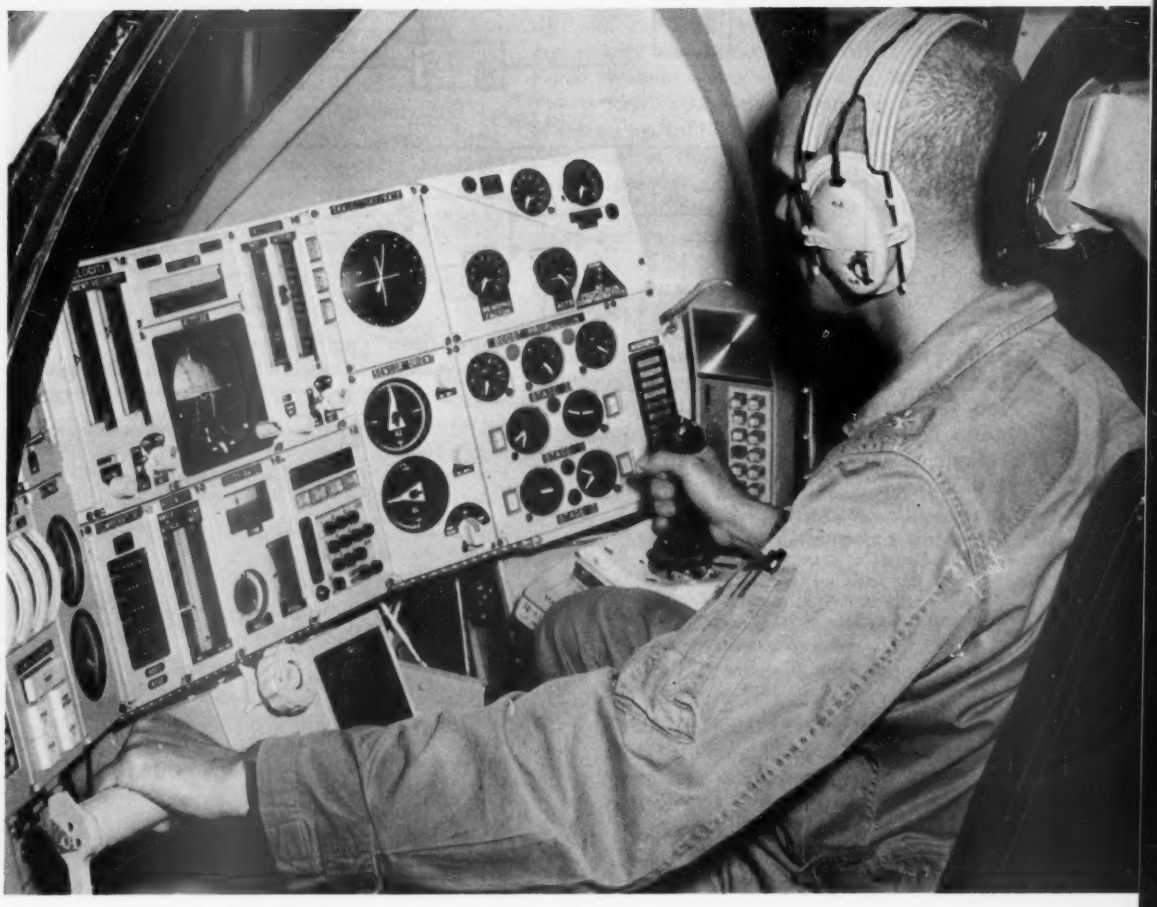
The end of Phase I of the Experimental Test Pilot Course signals the departure of the foreign and civilian students. As graduates of this phase, they have been trained to perform as test pilots, engineers, and managers on atmospheric test programs of research, test, and production-type aircraft and V/STOL vehicles. The U.S. military students remain at the school for an additional four months' training in the Phase II Aerospace Research Pilot Course.

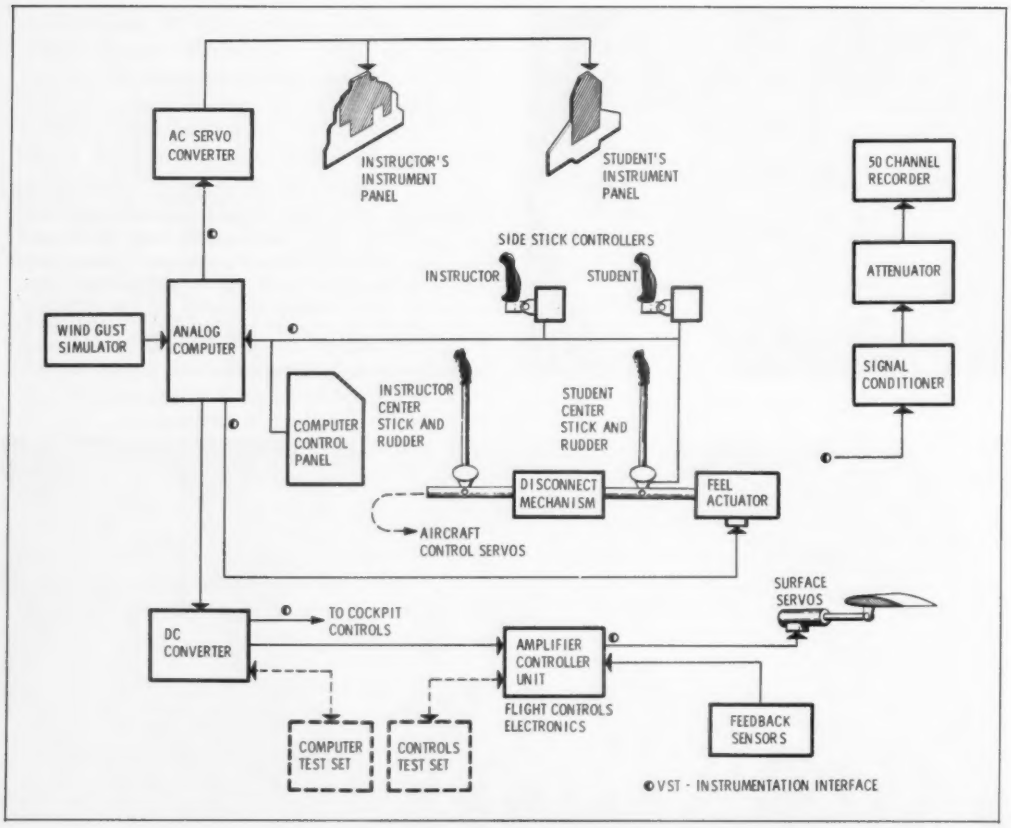
Phase II - Aerospace Research Pilot Course

The 3½-month Aerospace Research Pilot Course is designed to increase the qualifications of the Phase I graduate to enable him to participate in the flight-testing, engineering design, and/or management of the various Air Force advanced aerospace research programs. The course includes (1) academic instruction in subjects related to performance and operation of advanced aircraft and space vehicles,

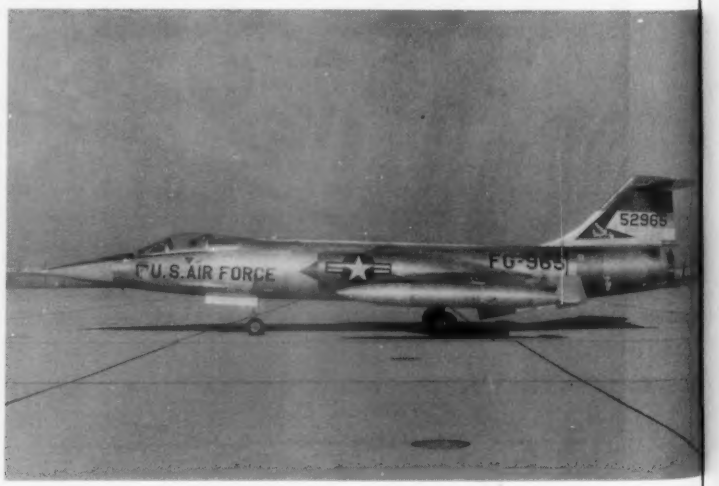


Students at the ARPS use simulators for practicing evaluation of control systems and specified tasks associated with aerospace/research vehicles. In addition, part tasks such as launch, rendezvous, and re-entry can be accomplished in the control systems simulators. . . . The T-27, which was used in the curriculum until September 1969, is equipped with a visual system, rendezvous vehicle, and special effects for simulations ranging from light aircraft to complex space missions.





The variable stability system of the NF-106 makes it an in-flight simulator of a variety of advanced aircraft and flight vehicles. . . . The F-104C is flown solo in the final five zoom missions designed to expose the student to the demanding requirements of flight in near outer space conditions.



(2) practical test flying in aircraft and simulators exhibiting the characteristics of research aircraft and manned space vehicles, (3) familiarization with the physiological and psychological aspects of high-performance and space flight, and (4) field trips to various government and civilian facilities engaged in the development, testing, and employment of present and future advanced aircraft, space vehicles, and related systems and components.

academic program

Much emphasis is placed on the intensive Phase II academic program. The subjects are presented at the graduate level and consist of background studies in astronomy, digital computers, bioastronautics, space environment, and the supporting mathematics. The mechanics and performance aspects of advanced aircraft and space flight are investigated in courses on rocket propulsion, space flight mechanics, and atmosphere re-entry heating. Finally, aircraft and space vehicle guidance and control capabilities are studied in space navigation and inertial guidance courses.

simulation

An extensive simulation curriculum bridges the gap between classroom theory and practical application. The missions are keyed to progression in academics and range from small part tasks in fixed base devices to full mission simulations including complete propulsion, control, guidance, visual cues, instrument displays, sound, heat, pressure suit facilities, vibration, and simulated motion and g levels, etc., of a specified space system in real time. Most major maneuvers of the current and near-future space programs are duplicated by simulation, using currently three kinds of simulation equipment:

Static Aerodynamic Simulator – used to simulate any aerodynamic vehicle, including those with rocket propulsion and reaction controls, at any altitude and velocity, from re-entry to light aircraft environment.

Control Systems Simulator—used to simulate any aerospace mission part task from attitude orientation and control evaluation to launch, rendezvous, and re-entry.

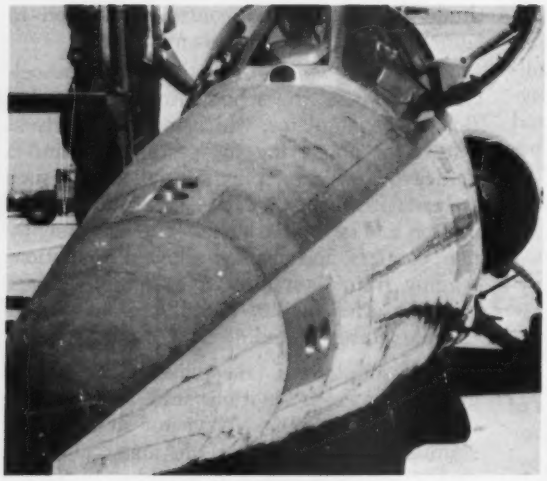
T-27 Simulator—Until the recent cancellation of MOL, the school used the complex T-27 space simulator, which included a visual system for portraying the earth, stars, and a rendezvous vehicle, plus special effects such as heat, pressure suit facilities, sound, and vibration and vestibulary cues. The instrumentation, controls, and displays allowed simulations ranging from light aircraft to complex space missions. This facility is still present, but in "flyable storage" condition.

All simulations take place under normal gravity; however, motions and attitudes are used to place the gravity vector so as to best simulate the g forces of the mission. Actual zerogravity experience is provided in an Air Force Systems Command C-135, flying zerogravity and moon-gravity profiles. Heavy g profiles (up to 15 g) are experienced in the centrifuge at the School of Aerospace Medicine, Brooks AFB, Texas, where actual space-program g profiles are flown, including launch, abort, and re-entry.

flying

The Phase II flying curriculum fortifies the flight-test training of Phase I by providing additional experience in high-performance and unusual aircraft. It also provides space- and research-related flight experience by using special and conventional aircraft in configurations with research-vehicle handling qualities in typical research flight profiles. This experience includes exposure to zerogravity, pressure suit survival, rocket propulsion, reaction control handling, energy management, variable stability, and lifting-body flight profiles and landing characteristics.

Included in missions flown in the Phase II curriculum are low lift-to-drag ratio (L/D) patterns and landings. In a series of about a dozen flights the student examines the handling and performance characteristics of an F-104 in various low L/D configurations designed to simulate X-15 and lifting-body vehicles. Several profiles are flown to include the X-15 landing pattern and high- and low-speed lifting-



Several pilots of each ARPS class fly three additional zoom missions in the NF-104, which is an F-104A modified by adding a rocket engine, extra wingspan, a larger tail, reaction control jets in the wingtips and nose, and a reaction jet controller in addition to the center stick.

body landing approaches. In addition to becoming familiar with the limited performance characteristics and precise energy management requirements of low L/D vehicles, the student derives practical experience in determining the L/D's of variously configured aircraft; he can then predict the critical parameters for vehicle maneuvers and landing patterns. Although only a few graduates of the school will be fortunate enough to fly or test vehicles such as the X-15 or the lifting bodies, a much larger number will be associated with advanced aircraft programs in support, chase, and managerial positions. These considerations, in addition to the test pilots' enhanced capabilities resulting from the varied experience, make the low L/D program an extremely useful portion of the curriculum.

NF-106. The NF-106 variable stability trainer (mentioned in the Phase I discussion) is designed for extensive application in the Phase II curriculum, which is oriented towards advanced aerospace research vehicles. It is a two-seat F-106B with major modifications that allow it to be used as a "model follower" in-flight simulator of a variety of aircraft or flight vehicles. The modifications include a 144 amplifier analog computer, which stores the model to be followed, a stability parameter programmer, a force feel system for the aft cockpit center stick, a front and rear cockpit side-arm controller, and an auto pilot that follows three parameters—vertical acceleration, side acceleration, and bank angle under cruise conditions—and automatically flies the NF-106 in such a manner that the model and actual aircraft parameters are equal.

To simulate a desired aircraft, the equations of motion of that aircraft are first programmed into the analog computer. The aft center stick is disconnected from the basic NF-106 and connected to the computer and force feel hydraulic actuator. As pilot control inputs are fed into the computer, the equations of motion are immediately solved, and the expected motion of the simulated aircraft appears at the output of the computer. The student's instruments in the rear cockpit, which receive this computer output, then indicate the airspeed, altitude, attitude, etc., of the model. An electronic loop which monitors the vertical acceleration, side acceleration, and bank angle of the model automatically drives the flight controls of the NF-106 in such a manner that

these three parameters are the same as in the model. The hydraulic feel actuator then works on the aft center stick to give the pilot the same forces he would feel were he actually flying the aircraft being simulated.

The stability parameter programmer can be used to change the handling qualities as a function of mach, airspeed, or altitude. This allows the NF-106 to simulate other aircraft for any portion of their flight profile.

The school's two NF-106s are used in a unique series of missions. The Phase II portion of these missions includes investigation and evaluation of

- NF-104 energy management and stability and control profiles
- X-15 high-altitude profiles
- X-15 high-speed profiles
- X-24A lifting-body energy management flights
- X-24A lifting-body stability and control flights.

The NF-106 enables the student to experience and evaluate the flight characteristics of the selected vehicle through any portion of its flight profile. For example, the X-15 high-altitude mission takes the student from launch at 45,000 feet altitude, mach .84, and a dynamic pressure of 145 pounds per square foot to a peak altitude of 250,000 feet, velocity of 4460 feet per second, and a dynamic pressure of .7 psf. The simulation then continues to a high-dynamic-pressure re-entry condition above 75,000 feet. Although the NF-106 itself will not approach this profile, the student experiences all the handling qualities, instrument readings, and visual and acceleration cues (within the limits of the NF-106) that he would experience in an actual X-15 flight. The simulation can be frozen at any point in the profile, to allow the student to perform a detailed examination of the handling qualities at that point. It is anticipated that the learning outcomes of these simulations will be of great value to pilots participating in present or future advanced research and space flight programs.

Zoom missions. Probably the most eagerly anticipated and rewarding missions in the Phase II curriculum are the zoom familiarization flights. In addition to the great benefits of broadened experience, these missions are designed to expose the student to the demanding requirements of flight in near outer space conditions. The basic program consists of seven missions flown in the F-104. During the first mission, pressure suit familiarization, the student flies a depressurized F-104 at high altitudes and high mach numbers to experience the techniques and limitations of flight in a fully inflated suit. The second mission, a demonstration of the basic zoom profile, is with an instructor and is flown in a two-place F-104D; it is limited to a 30° climb angle with a resulting peak altitude between 70 and 80 thousand feet. The remaining five missions, flown solo in an F-104C, are basically the same maneuver with climb angles increasing from 30° to a maximum of 45°. Resulting peak altitudes, which are a function of exactness of technique and meteorological conditions, average just below 90,000 feet. The engine is turned off at approximately 75,000 feet and restarted when descending through 60,000 feet, and a precautionary X-15 type pattern is flown to the Rogers lakebed runway at Edwards. In the eight years this program has been in existence, there has never been a case of failure of an engine to restart.

NF-104. For a portion of the class, completion of the F-104 zoom missions signals the end of the flying curriculum. However, a select few of the students in each class are given the opportunity to fly an additional three-mission NF-104 zoom program. The NF-104 is an F-104A modified by the addition of a rocket engine, extra wingspan, a larger tail, reaction control jets in the wingtips and nose, and a reaction jet controller in addition to the center stick. The aircraft is designed to fly basically the same zoom profile as the F-104 but with a significantly higher altitude capability. This higher altitude is reached with the help of a 6000-pound-thrust JP-4/hydrogen-peroxide-fueled LR-121/AR-2 rocket engine, which is used on the run-in and during the climb portion of the mission. Thrust duration is approximately two minutes.

The altitude capability, using a mission-prescribed climb angle of 40°, is between 100 and 110 thousand feet. At these altitudes the dynamic pressures are low enough (20 psf) to allow precise aircraft attitude control through the use of hydrogen peroxide reaction control jets. Manual actuation of the jets is by a hand-operated reaction control handle.

The F-104 and NF-104 zoom missions give the student unique experience in a variety of disciplines oriented toward advanced aircraft and research vehicles. In addition to familiarity with basic X-15 type flight, re-entry, and landing profiles, the student gains a rare appreciation for the precision, energy management handling, and meteorological tradeoffs involved in safely reaching the maximum altitude capability of an aircraft. Aircraft response to normal and reaction controls at extreme altitudes is investigated. Zerogravity conditions are experienced. Pilot capabilities and limitations under full pressure suit operation are demonstrated. Finally, the missions serve as an evaluation of the pilot's ability to react optimally under the demanding conditions of environment and vehicle performance to be found in present and future advanced aircraft research programs.

The end result of the zoom missions is a pilot who has experienced, as closely as economically and operationally practical, many of the conditions and exacting requirements of advanced research flight. This experience, added to his test pilot training, is an invaluable asset in the actual manning, support, or management of future manned or unmanned flight research and space programs.

The X-24A lifting-body research vehicle is one of the aircraft simulated in the school's NF-106 missions, for investigation and evaluation of its energy management flights and its stability and control flights.

As AT THE END of Phase I, the students finishing Phase II are taken on several field trips to industry and government agencies, to broaden their knowledge of the hardware and techniques of advanced aircraft and space operations. One of the major excursions is a week-long orientation at the School of Aerospace Medicine, Brooks AFB, Texas, to examine the physiological and psychological aspects of high-performance flight and space flight.

Graduates of the combined Phase I and Phase II course are uniquely qualified to take part in both atmospheric and space-oriented test programs. They are also thoroughly prepared to undertake the highly specialized training required for mission performance as astronauts in specific space vehicles.

GRADUATES of the Aerospace Research Pilot School have taken part in most of the significant flight-test achievements in this and several other countries. They fill assignments as research pilots, key crew mem-

bers, and managers in Air Force programs, including every modern bomber, fighter, and transport, the X-15, YF-12, and B-70, the lifting bodies HL-10, M2-F2, and X-24, and the Mercury, Gemini, Apollo, and Manned Orbiting Laboratory (MOL) programs. (In February 1966 ARPS developed a preliminary MOL crew training plan for the Space Systems Division of Air Force Systems Command. All MOL astronauts were to be graduates of ARPS and of the special 4- to 6-month postgraduate course in subjects directly applicable to the MOL. Three groups completed this training before cancellation of MOL in mid-1969.)

While 41 of the nation's astronauts are graduates of ARPS, the majority of the school's military graduates go on to positions as research pilots in the Air Force Systems Command. Typical of their assignments are positions in fighter or bomber test operations at the Air Force Special Weapons Center, Kirtland AFB, the Air Force Missile Development Center, Holloman AFB, the Armament Development Test Center, Eglin AFB, the Aeronautical Systems Division, Wright-Patterson AFB, and the Air Force Flight Test Center, Edwards AFB.

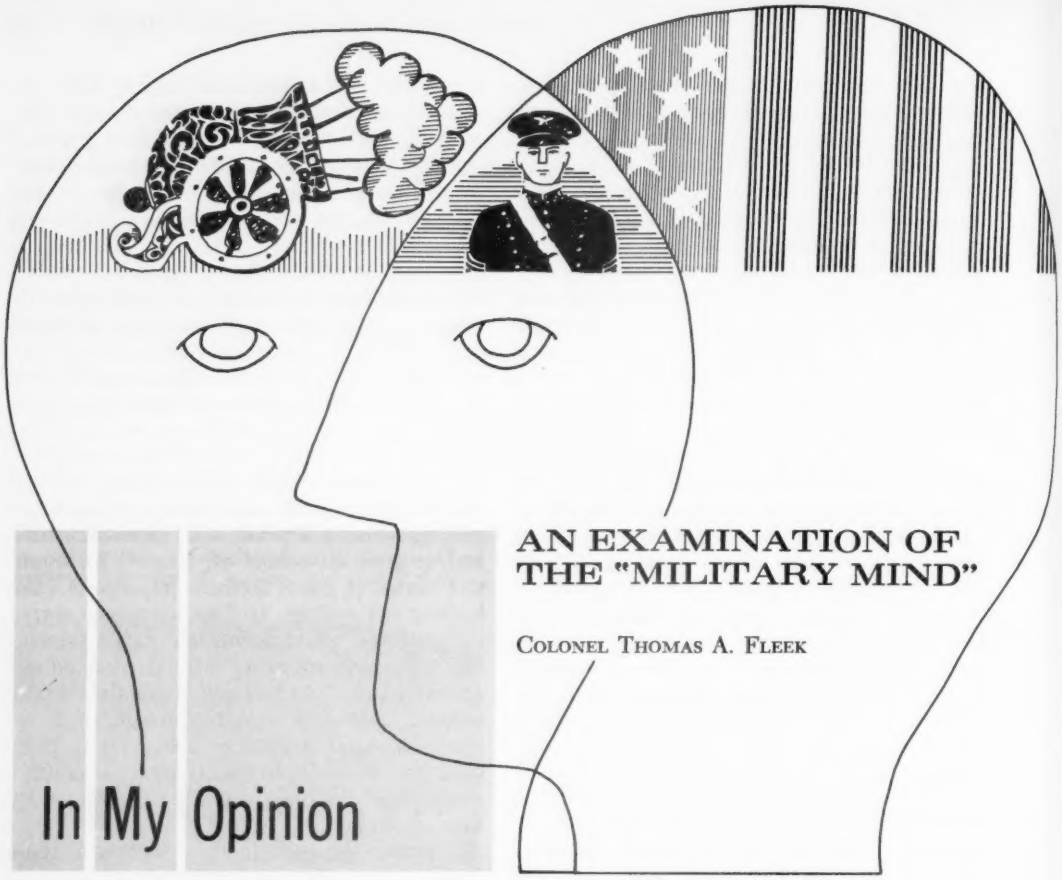
The staff of the Aerospace Research Pilot School consists of highly qualified Air Force test pilots, scientists, and engineers. Although none are specialists in *every* field associated with flight test, the overall composition of the staff ensures that each of these areas is covered by at least one instructor with expert qualifications. In addition to experience in bomber, fighter, transport, and V/STOL test operations, the staff as a whole has had considerable combat, scientific research, and educational experience. Total flying time per staff member averages over 3500 hours, and among the 28 working instructors there are twenty master's degrees and two Ph.D.'s in related scientific disciplines.

The general aim that dictates the school's future direction is to maintain a dynamically progressive curriculum which will stay ahead of the anticipated needs of future atmospheric and space test programs. This requires continually changing courses of instruction, development of new test methods, and flight-test training in the latest state-of-the-art vehicles.

The school's academic flight-test curriculum is progressively updated by selecting as staff members test pilots with recent scientific and/or graduate-school experience. An extensive series of guest lectures by experts from leading universities, civilian aerospace corporations, and government test facilities augments the modernization of the curriculum and provides an insight into new methods of flight-testing. Major new equipment under study includes a variable stability helicopter, a practical and reliable V/STOL training aircraft, a powered lifting-body trainer, and a universal aircraft simulator.

Proper direction in the selection of these new vehicles, the timely changes to the curriculum, and the development of new test methods are ensured through close and continuing contacts between the Aerospace Research Pilot School and the many branches of the aerospace profession.

USAF Aerospace Research Pilot School



In My Opinion

AN EXAMINATION OF THE "MILITARY MIND"

COLONEL THOMAS A. FLEEK

IN THIS period following World War II, and particularly in the recent years characterized by a preoccupation with cost effectiveness and management analysis techniques in defense policy formulation, there has been an interesting by-product of our military evolutionary process. This has been the seeming establishment of a widespread belief that there is a particular type of mentality called the "military mind" and that it has some inherently undesirable characteristics.

Precisely what these characteristics are supposed to be, and how they originated, are questions that could stand a more detailed examination. Dispassionate evaluation, if possible, is always preferable to semantic reaction. During the past few years, the services have been obviously concerned with broadening and improving both the capabilities and the image of the military mind. If there has been little headway, particularly from the standpoint of prestige accorded military expertise, it would be well to find out why.

One obvious analytic problem that should make us suspicious of the validity of any general label lies in the complexity of the subject. The history of attempts at character and intellectual assessment is a long one, and the various methods that have been undertaken at one time or another illustrate the difficulties and uncertainties of the task. Psychological tests, intelligence tests, personality inventory tests, psychiatry, palmistry, phrenology, handwriting analysis, astrology—almost every possible mea-

surement technique has been tried in one field or another. Yet it is unlikely that any of them or all in concert have ever enabled anyone to define and analyze exactly the mental makeup of a human being, let alone identify a precise, mass mental orientation that can supposedly result in rigid intellectual or emotional uniformity.

But imprecise as these means of measurement may be, even phrenology might be preferable to what seems so far to have been the analytic method. It has apparently been assumed that the nature of the military mentality is adequately revealed by spoken and written expressions of military thought and by various public actions and pronouncements of those who either are or have been prominent in military affairs. Not only are such sources of data limited to begin with but they also give no assurance that those who have gained most public attention by their speaking and writing on military affairs have been the most able and intellectual.

As for the nature and amount of written matter available for analysis, journalist and military historian Walter Millis, as editor of *American Military Thought*,[1] highlights the inadequacy of this means. He points out that American military men, until quite recently, have not been given to philosophical reflection on their profession; that we have produced no Clausewitzes and not many who could rank with leading European theorists. American military literature has tended to concentrate on either military history or techniques, usually on controversial operational matters—weapons, tactics, organization, employment of forces, and so forth. He finds that expressions of the ideas of professional soldiers alone are too few and too limited to provide any representative coverage of the broad field of "American military thought."

His conclusions can easily be substantiated by even a cursory review of the many books on military strategy and policy that have been written during the past two decades. Most of the authors have been civilians. A good example is Morton H. Halperin's recent work, *Contemporary Military Strategy*.[2] In his acknowledgment of his great debt to his "fellow strategists" and his listing of the most important works from which he has drawn, there is a virtual absence of any military authors.

At this point, then, one early conclusion might be that if there is indeed a military mentality it is not exclusively an attribute of the military man himself. However, if we are to avoid the same hasty identification pitfalls into which others have tumbled, it is apparent that a more searching look into what might constitute the military mind is obviously required.

One of the most thorough reviews of military authority, influence, and ideology in our society is that by Professor Samuel P. Huntington in his book *The Soldier and the State*.[3] He has specifically identified and analyzed a particular intellectual orientation which he has labeled "the military mind."[4]

This orientation he describes as essentially an intellectual and professional one. He asserts that the military officer career is rightfully classed as a profession, since it definitely meets the principal criteria of professionalism, and that the central skill involved is what he calls "the management of violence." One may perhaps argue the use of this particular phrase, but it seems admissible if we agree that the application of military force, in any context and manner, does represent a form of controlled "violence."

He believes that this particular skill, if properly developed, is a highly intellectualized one. It requires a strong sense of social obligation; a general and broad educational background that permits a deep understanding of human attitudes, motivations, and behavior; a high order of expertise; and a strong professional motivation.[5]

As for the resultant mind which this profession develops (or perhaps requires—the chicken/egg analogy seems to apply), Huntington attributes the development of a professional military ethic to the period between the Civil War and World War I: an ethic which views war as a science—a separate field of study. Its philosophy is essentially conservative and it is characterized by a strong feeling of corporativism, the sense of belonging to a special group. His general assessment indicates that he considers it not so much anti-intellectual

as anti-idealist: marked by an innate pessimism, where human behavior is concerned, and having little faith in the inevitability of peaceful progress. Its highest values he describes as "subordination, loyalty, duty, hierarchy, discipline and obedience."

To digress a moment in this last regard, if one accepts these values as correct—and it is indeed unlikely that a military man will find much about them to quarrel with—there may lie one possible source of civilian mistrust of the military mind. As Professor Huntington points out, several of these values are directly opposed to the traditional American virtues of self-reliance, initiative, self-determination, and the exercise of individuality.

At the same time, Professor Huntington may have identified a source of military self-contradiction. An adherence to such traditional military values, especially in their most simplified and basic context, can conflict with the broad, intellectual view of what constitutes "professionalism." The principles of "subordination, . . . discipline and obedience" are, in effect, constraints, developed through conditioning, to ensure the smoothness and continuity of functioning of the many component parts of a single complex operation or entity. Where the individual exercise of technological skill or general functional competence is concerned, however, a strict insistence on such constraints and limitations may well have derogatory results. Not only can it inhibit creative thinking and effective operation; when the result is arbitrary decision-making, it can give rise to serious omission or error.

Thus it is that emphasis on the need for broad general and professional education, and the recognized need for technical competence, at least in the advisory stages of the decision-making process, actually tend to develop an opposition to rigidly authoritarian systems and hierarchic structures. In the drive for improved educational opportunities, more highly specialized technical skills, and broadened professional education in the service schools, it is likely that there have been set in motion powerful and far-reaching influences that will ultimately affect not only the roles and duties of officers but perhaps the military structure.

It should be recognized that this emphasis on the higher education of officer personnel has resulted from service recognition of the requirement. It is not something that is particularly new or that was recently forced on the services by civilian criticism of former viewpoints and capabilities. For example, it has been more than ten years since Professors Masland and Radway made a comprehensive review of military education and its relation to national policy-making, and their conclusions at that time were:

> The record of accomplishments of professional military education . . . is a record of which the armed forces justly may be proud; it is far better than many civilian educators and laymen realize.[6]

And:

> We conclude that military education does make a very substantial contribution to the preparation of officers for policy roles. Within a matter of a few years, it is likely that officers assigned to these positions will have had significantly more formal educational preparation than comparable civilian officials.[7]

It should be noted that those "few years" have now transpired, that many of the educational shortcomings which Professors Masland and Radway pointed out have since been corrected, and there has been even more emphasis on both professional and general education. At the same time, another of their predictions has been borne out:

> . . . military officers will continue to participate in the formulation and execution of the most diverse national security policies and programs until national security itself ceases to be a paramount concern of American policy.[8]

In reviewing military efforts to prepare officers to better meet their new and complex responsibilities, Professors Masland and Radway also analyzed what they considered the primary requirements of military professionalism:

- First, military competence, because this, after all, constitutes the military man's special expertise.
- General executive ability—skill in lead-

ership, the ability to evaluate information quickly and accurately, to establish harmonious working relationships, to communicate effectively.

• The ability to grasp complicated situations, to see the "big picture," to adapt creatively to changing situations.

• A lack of parochialism and a strong sense of job motivation.

It is apparent that there is military agreement with this analysis of requirements, since many of them bear a marked resemblance to the rating factors listed on present Officer Effectiveness Reports. This cannot, however, be taken as proof that the selection process is bringing the military manager ever closer to some goal of ultimate perfection, since admittedly the effectiveness of the system is still limited by traditional hierarchial considerations. For example, a senior officer who has spent a career at base level, absorbed in a variety of daily operational details and preferring this type of duty, may well be rated as highly in all respects as another who has spent much of his career in high-level policy-making areas and activities. Equal rank supposedly makes them equally capable as "general managers." Yet there may well be a world of difference in their viewpoints and intellectual capabilities.

CONSIDERING now all the foregoing traits, characteristics, and requirements, we should be able to go back and begin sketching in what we have so far developed as the characteristics of at least a theoretical model of the military mind. It must be emphasized that we do not do so in the sense of making any value judgments. Whether these characteristics are good or bad, desirable or undesirable, and whether and how much they are influenced by basic intelligence differences—these are not the main issues. We are discussing a theoretical model. Our main concerns should be whether or not such a model reflects the characteristics required for the proper exercise of the profession and whether it recognizes and provides for necessary development and adaptation. This last consideration appears to be particularly important, for as a former Commandant of the Air War College pointed out:

> The military profession, like our whole civilization, is being carried along by the galloping advance of science. The consequence must be a fundamental change in our thinking habits.[9]

In summary, then, these appear to be the main characteristics of our theoretical model of a military mind:

First—and properly—there is a high concern with professionalism. But despite constant reiteration that military men must become better fitted to participate in high-level policy-making, the interpretation of "professional" still tends to be a limited one. It is likely to be equated with technological skill—with functional competence. This emphasis on the unique and complex nature of special military skills may be a compensatory device, resulting from an apparent neglect of the military man and a corresponding reliance on the civilian expert as a source of more high-level military expertise.

Perhaps as a result of this interpretation, there is a preoccupation with operational matters, to the exclusion of any significant amount of theoretical or philosophical reflection on the science of warfare as a whole. This tendency is self-reinforcing, for it in turn fosters parochialism and encourages arbitrary delineation of roles and missions—factors which, in themselves, tend to limit any analytical or theoretical exploration.

An "inward-oriented" tendency is further encouraged, if not forced, by hierarchial and corporate considerations and constraints. These include the equating of rank with authority and decision-making responsibility, the inherent orthodoxy that characterizes any corporate establishment, and the essentially conservative, rational, and realistic view of human nature and human affairs that is necessitated by the nature of military functions and responsibilities.

There is a growing realization of the need for greater adaptability, for adjustment to change, despite a consequent weakening of traditional military methods and systems. At the same time, the organization, structure,

limits of authority, and functions of the military establishment are primarily civilian-determined and -directed. Thus the limited military capability to influence change, despite a recognition of the need for it, can be a real source of frustration.

There is a firm acceptance of the principle of dedication to public service, a perhaps oversimplified but idealistic view of the traditional "soldierly" virtues, and a pessimism and mistrust concerning the more complex liberal-intellectual values. Also, there exists a source of inner conflict in the realization that true professionalism, at the high "generalist" level of management, requires a more liberal-intellectual orientation, a broad, thorough general and professional educational background covering modern management techniques, national security planning and policy-making, and the general conduct of national affairs.

Hence, the theoretical military mind, if there is such a thing, is certainly not a mentality for which one should be apologetic, but at the same time it must be subject to a high degree of frustration. There are powerful conflicting drives, conflicting forces, presently at work within the military establishment itself, as well as within the individual. Faced with what is apparently a pressing need for profound and far-reaching changes, whose exact nature and requirements are ill-defined at best, the military man finds many new concepts at odds with the traditional view of the military role, responsibilities, and organization. At the same time, even as he is assured of the importance of his counsel in the conduct of national affairs, the apparent supremacy of civilian thought and expertise in his own field—the broad area of military strategic and conceptual planning—has further downgraded his intellectual image and self-esteem. As a result and in self-defense, he has tended to equate military professionalism with the performance of those special military skills in which he can claim unquestioned supremacy. He has, in effect, resigned himself to the role of operator and tactician, rather than strategist and theorist.

Whether this trend will, in time, be reversed, or even whether it should be, are not matters with which I intend to concern myself at the moment. It does appear, however, that attempts to raise the general educational level, to prepare military personnel for broad planning and policy roles, and to elevate military expertise to a recognized high level of importance are being frustrated by many of those same constraints and limitations that I have described as affecting the individual himself.

It also appears that if we are to realize the full potential of the mind of the military professional and make the best use of its inherent virtues and specialized capabilities we must resolve the present main sources of frustration and restraint. How this can best be done is a complex question.

If the military profession is indeed a science, then it is probably subject to the same developmental processes as other sciences. It has been said that we cannot plot the course of scientific advancement in any field as a smooth, steadily rising curve; that instead, sudden, marked advancement is experienced only under conditions of "creative anarchy."[10] If such is indeed the case, and it appears to be, then the great question facing the military establishment today may well be how much creative anarchy can be permitted and how can it be encouraged and still limited?

It is a question well worth pondering.

Headquarters Pacific Air Forces

Notes

1. Walter Millis, ed., *American Military Thought*, Center for the Study of Democratic Institutions (New York: Bobbs-Merrill Co., 1966).
2. Morton H. Halperin, *Contemporary Military Strategy* (Boston: Little, Brown & Co., 1967).
3. Samuel P. Huntington, *The Soldier and the State* (Cambridge: Harvard University Press, 1964).
4. *Ibid.*, p. 258.
5. *Ibid.*, pp. 11–18.
6. John W. Masland and Laurence I. Radway, *Soldiers and Scholars—Military Education and National Policy* (Princeton: Princeton University Press, 1957), p. 502.
7. *Ibid.*, p. 503.
8. *Ibid.*, p. 25.
9. Major General Arno H. Luehman, "The Changing Role of the Military Professional," address delivered 12 August 1965 to the Air War College class.
10. See Arthur Koestler's *The Act of Creation*, specifically Chapter X, "The Evolution of Ideas" (New York: Macmillan, 1966).

Books and Ideas

A HISTORY OF THE LUFTWAFFE?

HARRY R. FLETCHER

TWENTY-FOUR years have passed since Anglo-American air power obliterated the Luftwaffe, Hitler's youngest and most National-Socialist oriented service; and a comprehensive history of the German Air Force in World War II is still to be written. Immediately following the war, few persons in Germany or the United States were interested in writing about the conflict that had just been concluded. By 1950, however, the world situation had taken a dramatic turn: the cold war had transformed West Germany into a comrade-in-arms with the Western Allies, and the German currency reform of 1948 had opened the way for a remarkable economic recovery west of the Iron Curtain. With the easing of their more serious burdens, Germans began to reflect upon World War II and its aftermath and to seek clearer insights into the protracted and far-flung operations of the Wehrmacht, in which most of them had served. Meanwhile, a wartime generation of Americans had sufficiently recovered its composure to become interested in the events of the recent past, especially the war against Nazi Germany, a nation against which so many had fought but about which few really knew very much.

As was to be expected, historians, part-

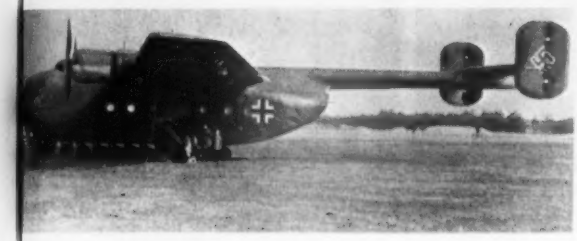
time historians, students, journalists, and freelance writers soon discovered that the writing of books about the war (particularly those dealing with the Nazis) could be both a fascinating and a lucrative enterprise. Dozens of works on World War II began to appear on the bookshelves here and abroad, the bulk of them being racy now-it-can-be-told accounts by eyewitnesses who were in the know. Replete with swastika-emblazoned dust covers and half-tones of diving Stukas and menacing Tiger tanks, the "histories" found an eager public quick to buy all that came off the press.

Books about World War II have a tremendous current sales-appeal, but it is also true that many of them are highly romanticized, filled with breezy jargon, and often superficial and downright misleading. It is safe to say that some of these works have more in common with the TV series featuring the Desert Rats or Colonel Klink than with any real events in history. Quite obviously, most of these books were not written as history; they were written to sell, and they have succeeded admirably.

Amid this deluge a number of significant and substantial military histories have emerged, including Walter Hubatsch's *Die deutsche Besetzung von Dänemark und Norwegen (The German Occupation of Denmark and Norway)*, Erich von Manstein's *Verlorene Siege (Lost Victories)*, and Albert Kesselring's *Soldat bis zum letzten Tag (Soldier to the Last Day)*, as well as the fine official histories published by Great Britain, the United States, and the Federal Republic of Germany. But, until recently, there had been no real effort to produce for public consumption a comprehensive account of the German Air Force in World War II, unless one cares to include Asher Lee's *The German Air Force*. The most useful single volume of this sort is the Air Ministry's *The*

Luftwaffe Heavy Aircraft

A Wehrmacht personnel carrier boarding a Messerschmitt Me 323 six-engine transport (left) . . . The Focke-Wulf Fw 200K (below), a long-range reconnaissance patrol bomber . . . The Arado Ar 232A transport (bottom).



Rise and Fall of the German Air Force (1933 to 1945), though this 1948 publication is still not accessible to the general reading public.

One of the more recent attempts to tell the story of the Luftwaffe to the layman is Cajus Bekker's *Angriffshöhe 4000 (Attack Altitude 4000),* published in Germany in 1964 and since translated into English in 1966 under the rather unfortunate title, *The Luftwaffe War Diaries,*† which the book obviously is not. Bekker's work is not derived from a critical examination of the surviving official war diaries of the former Luftwaffe but stems chiefly from a number of personal accounts, eyewitness reports, and several well-known German publications, such as Adolf Galland's *Die Ersten und die Letzten (The First and the Last,* Josef Priller's *Geschichte eines Jagdgeschwaders (History of a Fighter Wing),* and Werner Baumbach's *Zu Spät? Aufstieg und Untergang der deutschen Luftwaffe (Too Late? Rise and Fall of the German Air Force).* Happily, Bekker does not rely solely on these works but falls back on the USAF's *The Army Air Forces in World War II,* Denis Richards and Hilary St. George Saunders' *The Royal Air Force 1939-1945,* Basil Collier's *Defence of the United Kingdom,* and William Green's very good little series of aircraft identification books.

Bekker's work is not a historian's history; it lies midway between history and a compilation of eyewitness accounts. Yet it does have its place in the military literature of World War II and contributes a good deal to the general reader's knowledge of German air operations. General der Flieger (Retired) Paul Deichmann[1] is quite correct in noting that this is the first attempt in a single volume to tell the world about the Luftwaffe's operations on all fronts. For those interested in German aviation between 1933 and 1945, this book serves a purpose similar to that of William L. Shirer's *Rise and Fall of the Third Reich.* Both Bekker's and Shirer's volumes, despite their several defects, fulfill a need and render a service to the general public.

Bekker treats the theaters of operations separately, concluding each account with brief summaries and conclusions. The latter coincide well with the views expressed by German commanders who served in the particular areas mentioned and are historically sound as far as they go. What is lacking, however, is a proper emphasis upon root causes, upon the major fields of error and neglect prior to and during the war which affected the Luftwaffe, the incredible failure of Germany to mobilize its industrial power until the eleventh hour, and the all-too-seldom-mentioned fact that, in general, Germany's air leadership never comprehended the proper role of air power within the framework of modern warfare. Their strategic operations were, at best, tactical-strategic in character. This charge cannot be laid en-

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†Cajus Bekker, *The Luftwaffe War Diaries,* translated and edited by Frank Ziegler (Garden City, New York: Doubleday & Company, Inc., 1968, $7.95), 399 pp.

tirely at the feet of Hitler or the German Army. A much more perceptive assessment of the cardinal reasons for the decline and fall of the German Air Force in World War II is provided by Professor Richard Suchenwirth in his *Historical Turning Points in the German Air Force War Effort* (USAF Historical Studies No. 189, USAF Historical Division, Research Studies Institute, 1959; Arno Press, New York, 1968); and a clearer perspective of the instability within the High Command of the German Air Force can be gained by reading Suchenwirth's *Command and Leadership in the German Air Force* (USAF Historical Studies No. 174, USAF Historical Division, Aerospace Studies Institute, 1969) and Generalmajor Hans Detlef Herhudt von Rohden's *Das war die Führung (That Was the Leadership)*. Upon perusing these, the reader may with some justification ask whether, in view of the unique collection of "inadequate personalities" at the top level of the Luftwaffe, the organization was not doomed from the beginning. Perhaps it is symptomatic of this state of affairs that Goering, von Greim, Udet, and Jeschonnek all committed suicide.

Bekker wisely steers a middle course between the former one-sided adulation of the military and the equally narrow present-day German contempt for everything even remotely linked with the military, and it is refreshing to find an account dealing with German military operations that is not shot full of moralisms and in which the author does not manifest any signs of having a personal axe to grind.

ANOTHER recent work, John Killen's *A History of the Luftwaffe*,† attempts to tell the entire story of the rise and fall of the German Air Force from the days of Oswald Boelke, Max Immelmann, and Manfred von

†John Killen, *A History of the Luftwaffe* (London: Frederick Muller, Ltd., 1967; and Garden City, New York: Doubleday & Company, 1968, $5.95), 324 pp.

Nazi Notables

Reichsmarshall Hermann Goering visiting a fighter unit of the German Home Defense Forces, 1945 (far left) . . . The Inspector of the German Fighter Arm, Generalleutnant Adolf Galland (center) . . . Major Erich Hartmann, Germany's top fighter ace with 352 victories.

Richthofen to the collapse of the Third Reich. This is an ambitious undertaking, to say the least. Killen's book is not likely to be exciting reading, since it lacks the moving, personal accounts so plentiful in Bekker's volume. This would be agreeable enough except that Killen's contribution is hardly a solid piece of historical research. He avoids most of the recriminations so characteristic of books published during or immediately following World War II, but he tells the public almost nothing that it had not already discovered from the popular accounts mentioned in his all-too-brief bibliography: Galland, Hanna Reitsch, Baumbach, Shirer, and even Cornelius Ryan's *The Longest Day.*

More serious are Killen's obvious mistakes in fact and his willingness to abide by the long-discounted interpretations of a quarter of a century ago. For example, his treatment of the events leading up to Hitler's decision to intervene in Spain in behalf of General Francisco Franco in 1936 is clearly misleading. The air forces of the "two Spains" were not "about equal" at the outbreak of the civil war. The bulk of the Spanish Air Force threw in its lot with the Republic at the outset, and the Navy (largely because of the sympathies of its crews rather than any republican sentiments among the officer corps) did likewise. Moreover, with the exception of the impetuous Hermann Goering, none of the top Wehrmacht commanders were enthusiastic about suggestions to intervene in the Spanish War, and most of them saw grave dangers to an armed force—even to Germany itself—that had just been freed from the fetters of Versailles and had begun its buildup. The cardinal reason for German involvement in Spain was political, not military; Hitler saw an opportunity to keep the democracies (and even his troublesome ally, Italy) tied down in efforts to prevent an escalation of the conflict into a general European war while he pursued his political objectives elsewhere. Killen is satisfied to follow the stories, now largely discounted, that Hitler "and company" were instantly delighted to find a testing ground in Spain for their new arsenal. A number of solid and illuminating studies concerning the Spanish Civil War have been written since Hannes Trautloft's *Als Jagdflieger in Spanien (As a Fighter Pilot in Spain)* and Herbert Feis's *The Spanish Story.* Killen could have profited considerably from an examination of some of these recent publications.

A History of the Luftwaffe has a number of gross oversimplifications and inaccuracies, such as the comment: "But it should never be forgotten that he [Goering] became a powerful air leader only because Messerschmitt, Junkers, Heinkel, and the other German aircraft industrialists created the weapons for him to use." Goering's prestige as number two man in the Reich, as the last commander of von Richthofen's famous Fighter Wing No. 1, as former head of the Prussian State Police, and as President of the Reichstag was surely not attributable to weapon creators alone. Furthermore, his meteoric rise in military circles really preceded the mass production of combat aircraft. By 1937 Goering "had it made." It is regrettable, too, that Killen lumps Hugo Junkers and Willy Messerschmitt in the same category. Junkers, a man of strong moral conscience and basically a pacifist, was badly mistreated by Goering and the officialdom of the Third Reich, who wrested from him his

airline, his firm, his patents, his fortune, and were even attempting to try him for high treason when death benevolently intervened. Messerschmitt, on the other hand, was a great opportunist, a specialist in political intrigue, and a great favorite of Hitler, who considered him to be a genius. He retained this association until the end.

There is little to say of Killen's handling of the Imperial German Flying Forces of World War I, except to note that a reading of General Karl Bodenschatz's *Jagd in Flanderns Himmel (Pursuit in Flanders' Heavens)*, General der Kavallerie Ernst von Hoeppner's *Deutschlands Krieg in der Luft (Germany's War in the Air)*, the memoirs of Crown Prince Ruprecht of Bavaria, *In Treue Fest (Steadfast in Faithfulness)*, and the official history published by the Reich's Archives, *Der Weltkrieg 1914 bis 1918 (The World War, 1914 to 1918)*, would have been useful.

BY FAR the most swashbuckling and sensational book being examined here is *Horrido! Fighter Aces of the Luftwaffe.*† This book, which required twelve years of research, is written in large part to tell the world that the pilots of the former Luftwaffe were as honorable as those who opposed them in combat. Indeed, the fact that there is a broader recognition of a need to examine both Allied and enemy air forces in a saner and more considerate manner is commendable; the time for gross oversimplifications in assessing blame and making bland accusations is past. Yet there is a very thin line between honest respect for an opponent and an immature, fawning, naïve admiration. In far too many instances Americans have found themselves almost unable to view the entities of "allies" and "enemies" in true perspective. Too often, in re-evaluating the enemy, the pendulum is allowed to swing all the way from a bitter, grudging respect to something closely akin to hero worship. This is definitely the tone of Constable and Toliver's book on the Luftwaffe aces. It needs to be said again: the German Air Force was not made up of "supermen" or aerial "knights" who rode their "steeds of the air into the lists." The Germans themselves know better than this.

Until the closing years of World War II, the Luftwaffe was composed of men who had had a considerable amount of military and flying training and who were unquestionably very able. It was perhaps inevitable that in a war of attrition some of them would live long enough to become highly skilled in their craft and run up outstanding scores. In fact, all things considered, the Luftwaffe pilot—providing, of course, he could stay alive—had the best chance of all to amass a string of victories against an ever increasing Anglo-American force in the West or against the improving but often quite inept Soviet Air Force. Because of pilot shortages and crucial air defense problems, the German Air Force was obliged to keep its experienced pilots in combat. For example, Hans-Ulrich Rudel, the noted Stuka flyer, completed more than 2500 combat missions. How many Allied pilots remained in the thick of it this long? Yet one must still concede that the Luftwaffe had some outstanding units and that it had the highest-scoring aces to be found on either side.

Hermann Goering and other Nazi leaders overlooked no opportunities to compare their "boys in blue" with Boelke, Immelmann, Werner Voss, and the legendary Manfred von Richthofen of World War I fame. There were certain differences, however. Boelke and his contemporaries grew up in a period of relative stability, of great traditions, and of high respect, which extended from the family circle and the church to the Emperor himself. These men went off to war in 1914 for "God, Country, and Kaiser," holding fast at least to some part of the concept of knightly conduct—something which was even then passing from the scene (though many of them did not realize it). On the other hand, most of the Luftwaffe pilots grew up in a period of unrest, discontent, hun-

†**Trevor J. Constable and Colonel Raymond F. Toliver, *Horrido! Fighter Aces of the Luftwaffe* (New York: Macmillan and Company, 1968, $7.95), 348 pp.**

Luftwaffe Fighter Aircraft

Ground crew readying an Me 109 for action against the British (top) . . . One of Germany's best, the Fw 190 (center) . . . Fighter that came too late, Me 262 jet.

ger and inflation, frustration and envy. Most of them found their self-esteem in the promises of National Socialism, a philosophy which not only condemned representative governments for their weaknesses but also wholeheartedly attacked the more worthwhile and civilizing aspects of old Imperial Germany. Moreover, the Nazis emphasized such precepts as "even the score," *Gemeinnutz vor Eigennutz* ("community good before the individual good"), and "the end justifies the means."

German flyers of World War II were pragmatists of the first order, and there is no solid evidence to support the contention that they were somehow more chivalrous than Allied pilots. On occasion both sides were apparently guilty of shooting down airmen in parachutes, even though Constable and Toliver make the fantastic claim: "It is doubtful indeed if a German pilot ever strafed a parachuting enemy, although in the heat of battle anything can happen." (p. 33)

In their discussion of the bombing of Dresden, Constable and Toliver assert (doubtless on the strength of David Irving's book, *The Destruction of Dresden*) that American "red-nosed Mustangs" carried out strafing operations against civilians at Dresden. (p. 288)[2] Yet no documentary evidence has come to light to substantiate any strafing there by an American air unit, let alone "red-nosed Mustangs." Unauthenticated statements of this sort may lend nice bits of color to a book, but they scarcely add anything to its historical validity.

Despite the authors' description of hapless German flyers, who hated the Nazis and felt themselves "powerless to remove the political regime," (p. 300) it should be noted that most German pilots went through the Hitler Youth glider schools, the secret flying school at Schleissheim, and were in one way or another associated with the National Socialist Flying Corps before entering the German Air Force. Though it could be said that most of these

young flyers never were particularly interested in governmental or political matters, they were hardly "caught up" in the service involuntarily. Things were quite different for the Army and, by the close of the war, even for the *Waffen SS* (Military SS), which had to resort to conscription to fill its ranks. Finally, to the statement that German pilots "were caught up in World War II," one feels impelled to ask, "Wasn't everyone?"

The outstanding figure in this book is the former German Inspector of Fighters, Generalleutnant Adolf Galland, probably because few former Luftwaffe officers have been so delighted to be interviewed by Americans and because few of them have said so much. The authors' description of Galland as a "visionary genius" with "exceptional equilibrium" who eschews the "limelight" and has great personal modesty would be quite sufficient to place him among the greatest air commanders of his day, but their further eulogy of him as a man who "will go down in history as the Billy Mitchell, the Douhet, and the aerial Clausewitz of the Second World War" (pp. 24–25) can only lead the reader to conclude that Galland must have been easily the outstanding air force leader of all time. It is difficult indeed to imagine just how Constable and Toliver happened to select Galland as the outstanding Luftwaffe commander, let alone as an officer who must have outstripped in ability and achievements all the Allied air commanders as well. Several former Luftwaffe commanders have written opinions concerning leadership in the German Air Force, and most agree that Galland was indeed a highly competent junior commander, but not the "outstanding mind" in the Luftwaffe. An article appearing in the November-December 1946 issue of *Interavia* is even less commendatory, describing Galland as "just a typical example of the flying generation created by Hitler."[3] Like many of his contemporaries, Galland was never averse to ascribing all of the German Air Force's woes to the corruption of German political leadership. Yet he remained one of the Fuehrer's favorites almost to the end, and his jet organization, JV-44, was established only because Hitler agreed to its formation.

German fighter pilots may have developed a "contempt for the political leadership," but there is no evidence that they ever hesitated to carry out their assignments. Furthermore, it is interesting to note that, of the regular German branches of service, it was the Luftwaffe that manifested no interest in checking Hitler's ambitions before the war. During the 20 July 1944 attempt to assassinate Hitler, the Luftwaffe was conspicuous only by its failure to participate. On the other hand, as early as 1935 Generalleutnant Walther Wever, the first chief of what was to become the German Air Force General Staff, declared, "Our Officer Corps will either be National Socialist or it won't be at all!"[4] In 1936 it was the German Air Force that wanted so eagerly to intervene in the Spanish Civil War, and in 1939 it was the Luftwaffe that provided the principal means of coercion to force that unhappy man, Dr. Emil Hacha, to surrender Czechoslovakia to Hitler. There was, in fact, a saying which contained more than a grain of truth: "Germany has an Imperial Navy, a Royal Prussian Army, and a National Socialist Air Force."

Constable and Toliver's book has a number of errors of omission and commission, many of which might have been avoided by an examination of the existing archival materials pertaining to the German Air Force in the United States, Great Britain, and West Germany. The volume contains few footnotes and no bibliography at all. It does have a number of interesting charts bearing the scores of Germany's day and night fighter aces and Knight's Cross winners, as well as a useful glossary of terms. Unfortunately, the value of these charts is diminished by the fact that no sources are cited. *Horrido! Fighter Aces of the Luftwaffe* is not a history of the German Air Force; it is not even a history of German fighter forces. Rather, it is a compilation of biographical sketches of some of Germany's top fighter pilots, including men such as Erich Hartmann, Gerhard Barkhorn, Heinz Schnaufer, and Johannes Steinhoff, together with a few of their more colorful and harrowing experiences. A great amount of space is devoted to victory credits and the compilation of credits, which is natural for those who are former fighter

pilots or members of fighter pilots' associations. In this area, Hans Ring of West Germany is probably as good an authority on German victory credits and comparative scores as anyone living. Of course, this is but a very small aspect of the overall picture of a great air force that went down to defeat in World War II.

German aviation buffs who are mainly interested in fighter activities and in the credits assigned to aces will find this work quite interesting, but for those who hope to understand the German Air Force in a broader context and to find the reasons for its failure, Cajus Bekker's *The Luftwaffe War Diaries* will be much more satisfying. Better yet, if time permits, one would benefit from a reading of the historical studies prepared by former key officers of the Luftwaffe for the USAF Historical Division. These monographs cover every theater of operations in World War II and all the most significant aspects of the German Air Force as an entity. Ten of these studies have also been published commercially by Arno Press and are well worth the time it takes to read them. Meanwhile, perhaps some ambitious soul will tackle the task which still stretches before us, the writing of a solid, comprehensive history of the German Air Force.

Aerospace Studies Institute

Notes

1. General Deichmann was Project Control Officer (1953–58) for the USAF Historical Division's German Historical Monograph Project and became the first foreigner to be honored with the Air University Award.

2. See also David Irving, *The Destruction of Dresden* (London: Kimber, 1963), pp. 151–52. Irving cites only a single, fragmentary account from an Eighth Air Force group report. The report, however, is inaccurate.

3. Major Evans, "Vom Gymnasiasten zum Fliegergeneral" ("From High School Student to Flying General"), *Interavia*, Vol. I, November-December 1946, p. 42.

4. Generalleutnant Heinz J. Rieckhoff, *Trumpf oder Bluff? 12 Jahre Deutsche Luftwaffe (Trump or Bluff? Twelve Years of the German Air Force)*, (Geneva: Verlag Interavia, 1945), p. 83. See also Generalleutnant (Retired) Andreas Nielsen, *The German Air Force General Staff*, USAF Historical Studies No. 173, Maxwell AFB, Ala.: USAF Historical Division, RSI, June 1959, p. 185. (The latter is also published by Arno Press, New York, 1968.)

HERITAGE OF THE GREAT DEPRESSION: WORLD WAR II

DR. GEORGE W. COLLINS

IN THE beginning was the Great Depression—and from thence all evils flow. It is the economic crisis of 1929–33 that Pierre Renouvin uses as the point of departure for his study of "the origins, the development, and the immediate consequences" of the Second World War.† M. Renouvin, born in Paris in 1893, has been a prominent scholar in the field of diplomacy and international relations for many years. A long-time teacher at the Sorbonne, he also has had intimate experience with warfare dating back to 1914–18, when as an infantryman he was twice wounded.

This book, originally published in 1958 as *Les Crises du XXe Siècle de 1929–1945*, was the final volume of *Histoire Des Relations Internationales,* an eight-volume series dealing with international relations from the end of the Napoleonic Wars to 1945. Although the book is now over eight years old, its translation into English will be a boon to students of the period. Renouvin has not written a detailed account of the decade and a half but instead has attempted "to highlight the most important of the diplomatic negotiations and to give a critical interpretation of them." Although the bibliography for this later edition is dated, containing only works published by 1959, its breadth is commendable. The author has used the published documents of the major European powers and the United States, as well as a broad international range of the printed memoirs and secondary works. The text is divided into two sections: the first deals with the origins of the war and the second with international relations during the war and the immediate outcome. The book is of particular value to those who may be familiar only with American writings on the diplomacy of this period.

Renouvin recognizes the intimate connection between the First and Second World Wars; nevertheless, he does not simplify it to a mere cause-effect relationship. Although the first war struck the intercontinental dominance of the European powers a severe blow—and much of the nationalist-independence movement of Asia and Africa stem from that development—he observes that by 1928 the tensions and problems of the postwar world had been largely resolved and that in political and economic relations the world seemingly was entering an era of conciliation. He cites the favorable economic conditions as well as the signing of the Kellogg-Briand pact as indices of comparative well-being and acceptance of the status quo—surely the grand illusion!

Disaster soon struck, however, and the lamps began going out in Europe with the economic crisis of 1929–33. There is nothing new in his account here; the relationship between the initial crisis in the United States and the subsequent spread of the depression across the Atlantic is well known. It is the significance of the depression, rather than the causes, which interests Renouvin, and that, he maintains, was profound. The shock waves from that crisis struck at the very heart of Western civilization, and what emerged was a new and more frightening nationalism which established the basis for the overthrow of the status quo and led directly to the Second World War. The depression was more than merely an economic calamity: the basic values of liberal and parliamentary government were questioned, and, as the states turned to national solutions of their problems, tariffs and other trade barriers were erected. Furthermore, the quest for economic independence led to a reawakened interest in

†Pierre Renouvin, *World War II and Its Origins: International Relations, 1929–1945,* translated by Rémy Inglis Hall (New York: Harper & Row, 1969, $8.50), x and 402 pp.

national expansion. Eventually a new Europe emerged, dividing into blocs of "have" and "have not" powers.

In his discussion of the major powers' reaction to the depression, Renouvin establishes the procedure which he follows throughout the book. After sketching a given international situation, he proceeds to analyze the various national policies, then interprets their causes and significance. His keen insights are the most rewarding elements of the book. Renouvin writes with a convincing candor and impartiality of the relations between states rather than between statesmen; there are no heroes and only one villain. His understanding of international situations is impressive. Not only are the policies of the major powers studied, but, when the issues are more local, he demonstrates equal familiarity with the situation in Austria, Poland, or elsewhere. He closely follows the shifts in national policy, e.g., the Soviet support of the principle of collective security after 1930 when the threat of German and Japanese ambitions began to jeopardize the Soviet position.

While recognizing the adverse effect of the Manchurian episode of 1931 and the Italo-Ethiopian war upon the status quo and collective security (developments made possible by the inability of Britain and France to maintain a common front), Renouvin believes that the real turning point came in 1935. The formation then of the "Stresa Front" between Britain, France, and Italy and a Franco-Soviet mutual assistance agreement apparently implied serious attention by the British and French governments to the problems of European unrest, particularly that of Hitler's Germany. Unfortunately those agreements soon proved to be ephemeral. The Stresa Front broke down with the Italo-Ethiopian war, while the failure to accompany the Franco-Soviet pact with a military convention left it ineffectual. After that it was all downhill—German rearmament and the occupation of the Rhineland, wars in China and Spain, and, eventually, the final moves of Hitler in Central Europe in 1938–39.

Who was to blame? Renouvin unequivocally attributes the war to Hitler.

> It appears to be almost incontrovertible fact that the Second World War was brought on by the actions of the Hitler government, that these actions were the expression of a policy laid down well in advance in *Mein Kampf*, and that this war could have been averted up until the last moment if the German government had so wished. . . . In examining the origins of this war, one becomes aware that the will of a head of government and a group of men is the dominating factor. They were carried forward in their design by an outpouring of emotion that they themselves promoted and by an organization that stifled any manifestation of opposition. (pp. 167–68)

Renouvin denies that there was any economic necessity for Germany to go to war; instead, he argues that its policy was motivated by the desire for power. He observes that other scholars reached similar conclusions and that there has been no "revisionist" debate as there was after the First World War. It should be noted, however, that since the original publication of this book, at least two important divergent views have been published. A. J. P. Taylor has stated that the irresolution of Britain and France in deciding upon policies of appeasement or resistance "helped to make the war more likely."[1] He also suggests that Hitler's insistence upon war with Poland late in August 1939 was to impress the German military staff and that the Führer counted, to the very end, upon the backing down of Britain and France and upon a negotiated settlement. The war finally came largely by accident, says Taylor. "Hitler may have projected a great war all along; yet it seems from the record that he became involved in war through launching on 29 August a diplomatic manoeuvre [a formal offer to negotiate directly with a Polish plenipotentiary] which he ought to have launched on 28 August."[2] Another, who has studied the question of the origin of the war from an entirely different tack, is Denna F. Fleming, who views the war as merely a phase in the long crusade against Communism, a crusade which he believes has been largely mistaken. By the late 1930s, he states, all the efforts of the Western powers were intended to divert Hitler to the East. Fleming, however, fails to account for the entry of Britain and France into the war after what he terms their sacrifice of Poland.[3] This delay in "revisionist" debate after World War

II is more likely related to the critical level of international tension in the postwar years, a tension far more intense than that which followed World War I.

While Renouvin holds that the ultimate guilt was Hitler's, he does not exonerate the other powers. In that assessment he implicitly argues for the "old diplomacy," when the major powers accepted responsibility for leadership in the affairs of Europe and through a balance of power maintained the peace. Where such powers have important interests, Renouvin argues that they have the ability and obligation to control events. Thus, he believes that in the 1931 Manchurian episode Britain, as the most involved European power in the Far East, should have taken the lead. Similarly, despite lack of support from Britain, France could have acted to prevent the military occupation of the Rhineland, and the circumstances justified action. Again, in regard to the *Anschluss,* he holds Italy accountable for failing to support Austria. And finally, once Britain and France belatedly adopted a firm position in August 1939, had the U.S.S.R. supported them, Hitler probably would not have provoked war. What was the role of the United States? It "remained on the sidelines." "At no time between 1933 and 1938 did the United States play the role in international relations that her economic primacy should have afforded her."

This recognition of the failure of appeasement in the thirties is part of the nightmare of the sixties. With the bipolarity of power, the danger of nuclear holocaust, and apparently no limit to serious international crises, when and how is intervention required? What are the real threats to world peace, or to the best interests of the United States? The Truman Doctrine "to support free peoples who are resisting attempted subjugation by armed minorities or by outside pressures" cannot be universally applied, but the converse concept of "fortress America" is no more realistic. American postwar policy in general has been one of response and of a pragmatic nature. Policy is formulated as issues arise, while direct involvement has been limited to areas along the peripheries of the bipolar powers.

The second part of the book follows the diplomacy of the war through to the victory of the Allies. Once the war was widened in 1941 with the entry of the U.S.S.R. and the United States, Renouvin maintains that the defeat of Germany was inevitable because of the preponderant resources of the Allied powers. The only possibility of defeat lay in the disruption of the alliance, an opportunity which Germany never vigorously pursued. Nevertheless, the coalition between the Atlantic powers and the Soviets was always uneasy, as each feared that the other might negotiate a separate peace. That remained a distinct temptation for the U.S.S.R. until the second front was established in France in 1944.

Of interest is Renouvin's interpretation of the Big Three conferences. Whereas most scholars cite the Yalta decisions as the most important, particularly for the postwar world, Renouvin writes that the key decision was made at Teheran in November 1943, when a tentative agreement to move the Polish boundaries westward was reached. He questions the soundness of that accord, granted when the Soviets were not in a commanding military position and appeasement was unnecessary. Through it, he argues, Roosevelt and Churchill opened "the way to the penetration of Russian influence in Central Europe." Here Renouvin's judgment seems unduly blunt. Had the Western powers only postponed the decision, the military course of the war would have forced it at a later date. To have steadfastly rejected the Curzon line would have jeopardized the coalition. Moreover, the Soviets' "interpretation" of the Yalta agreements in regard to Poland, as well as their occupation policies, shows that for purposes of their own self-interest they would never have condoned a central Europe unfavorable to their regime.

The concluding parts of this book do not measure up to the rest. Chapter 14, "The World in 1945," endeavors in thirty pages not only to recount the situation in Europe and the Americas in that year but also to trace the emancipation movements from the thirties to 1945 for much of Asia and the Middle East. Even Renouvin's skill at synthesis is unequal to such a feat. The final section of the book, "General Conclusions," is also dissatisfying. Here his

four-page sketch of the rise and decline of European influence from the Middle Ages to 1945 is so brief as to be almost useless. He then proceeds to discuss some of the fundamental elements for the study of change in society, elements also essential to the study of international relations. These are the "demographic and economic conditions, as well as currents of collective thinking." Besides these, the author reminds us that since the sixteenth century, international relations revolve around the activities of the state; that "the state initiates the contacts between people and civilizations."[3] It would have been better to introduce these concepts at the beginning of the book, to enable one to better understand Renouvin's approach to the study of international relations. Moreover, one could see more clearly how he emphasizes the role of the state and relegates other factors to lesser importance.

Earlier, his interpretation of the causes of the major depressions is questionable. In stating that the situation in 1929 was similar to the depressions of 1837, 1857, 1873, 1893, and 1907 in that the origin of those crises was American, he attributes too much importance to the nineteenth century economic influence of the United States. During that century the circumstances of the 1920s were reversed: the United States was the debtor nation, and the flow of credits was westward across the Atlantic. Certainly British economic policy was a major factor in the depression of 1837. The raising of discount rates by the Bank of England in the summer of 1836 resulted in the curtailing of investments in the United States. Furthermore, it occurred at a time when cotton prices fell because the British demand declined. As Bray Hammond aptly wrote: "... the British had stopped buying, had stopped lending, and expected payment of what was due them."[5] Again, in the depression of 1873, the effect of the dumping of British goods in the European market must be evaluated.[6] By the middle of the nineteenth century the interaction of European and American economic developments was quite complex: contraction or expansion of the European economy hardly can be attributed to one side of the Atlantic.

Although Renouvin's interpretation of international relations of 1929–1945 is not new, it is a cogent reminder of how drastically the world has changed. Today the problems of pre-World War II Europe seem much less significant than they did then; indeed in this age of nuclear threat they can almost be viewed with nostalgia. The war unquestionably did drastically alter the world position of Europe, particularly western Europe, whose intercontinental empires crumbled forever. M. Renouvin has underestimated the phoenix, however, which, with its material and demographic resources, technical competence, and outside assistance, has again demonstrated its recuperative powers. The recent change in French leadership affords new opportunity for western European cooperation and for the creation of a "third force" in international relations, affording greater opportunity for diplomatic harmony.

Wichita, Kansas

Notes

1. A. J. P. Taylor, *The Origins of the Second World War* (Greenwich, Connecticut: Fawcett Publications, 1961), p. xi.
2. *Ibid.*, p. 267.
3. D. F. Fleming, *The Cold War and Its Origins, 1917–1960*, Vol. I (Garden City, New York: Doubleday & Co., 1961), p. 95.
4. For elaboration of these views, see Pierre Renouvin and Jean-Baptiste Duroselle, *Introduction to the History of International Relations*, trans. Mary Ilford (New York: Frederick A. Praeger, 1967).
5. Bray Hammond, *Banks and Politics in America: From the Revolution to the Civil War* (Princeton: Princeton University Press, 1957), pp. 457–59.
6. Gordon A. Craig, *Europe Since 1815* (New York: Holt, Rinehart and Winston, 1961), p. 287.

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AWARD

The Air Universty Review Awards Committee has selected "In Search of an Aviation Environment Master Plan" by Major William R. Sims, USAF, and Captain Angelo J. Cerchione, USAF, as the outstanding article in the September-October 1969 edition of *Air University Review.*

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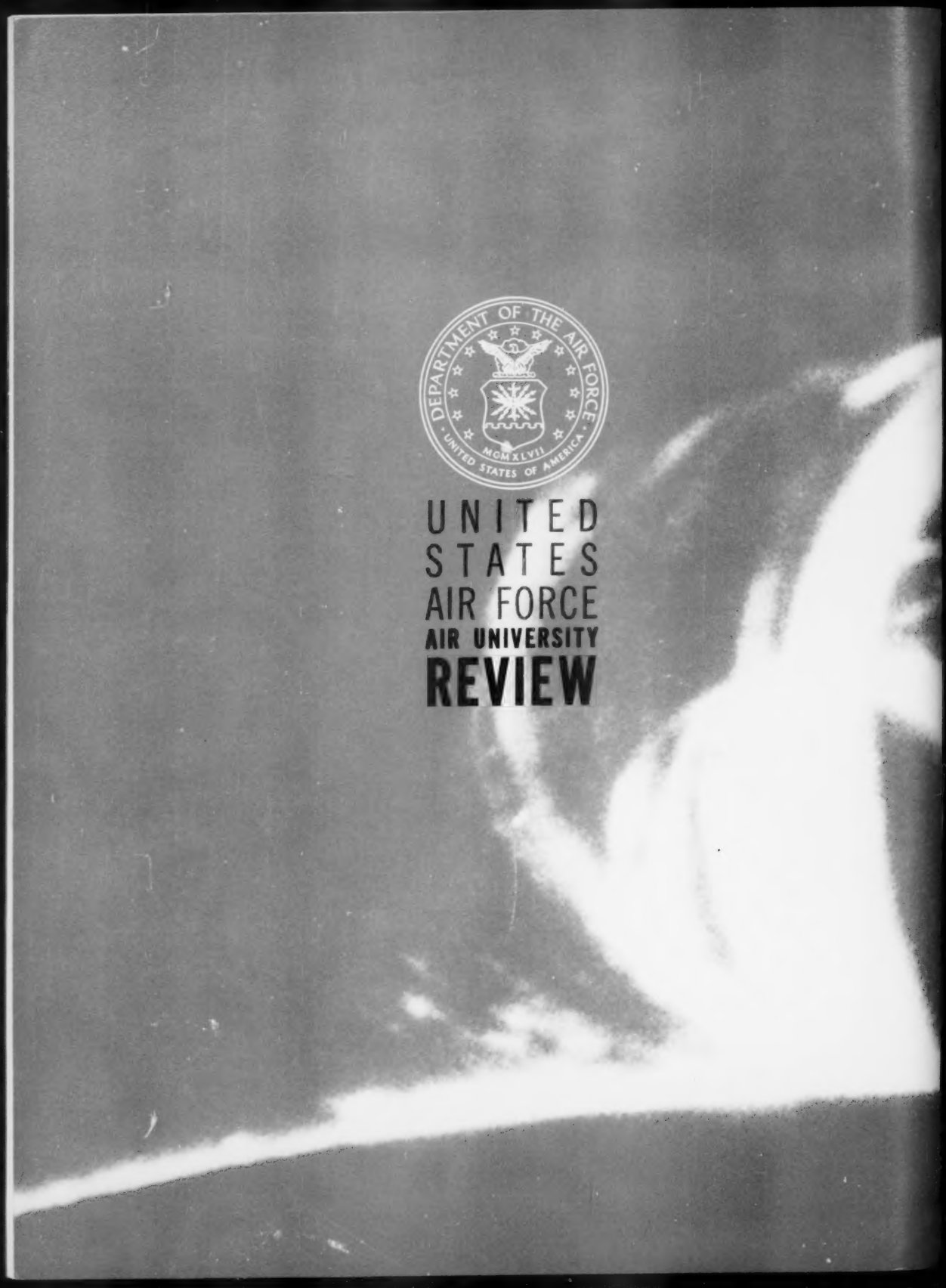

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